NATURE

No. 4156 SATURDAY, JUNE 25, 1949 Vol. 163

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The annual subscription rate is £4 10 0, payable in advance, inland or Abroad

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TRAINING THE RESEARCH WORKER

IN his annual report for 1947–48 to the Trustees of the Carnegie Institution of Washington, Dr. Vannevar Bush, president of the Institution, again refers to the appropriate relationship between the governing board and the scientific staff in such institutions, and particularly in regard to the shaping of programmes of research. Dr. Bush's discussion of these relations is of considerable general interest, particularly when greater attention is being paid to the organisation of research staff in industry and when the general conditions and conduct of academic research are being re-examined.

Dr. Bush at the outset recognizes frankly that the matter cannot be considered entirely without reference to political considerations. The division of the world into two sharply contrasted groups has repercussions in the field of science and of scientific research, if no more than as affecting the ideas which men hold about the freedom of science. Dr. Bush believes firmly that progress in pursuit of knowledge is made rapidly and securely only when individuals are free to entertain any hypothesis, and when the survival of hypotheses is determined solely by the rigour of test against cold facts and not by any external arbitrary rulings; and he finds it difficult to believe that such a system will not speedily outdistance any other system in which political dogma is allowed to dictate the teaching and practice of science.

Dr. Bush then emphasizes that, while programmes of research should arise from the integrated aspirations and judgment of the scientific staff, the question is more complex than at first appears. Due weight must be given to the imaginative power of the qualified mind; but something more is required than seeking out the unusual man and giving him untrammelled opportunity and generous support. In spite of all that has been accomplished by individual genius, the sound progress of a body of science requires even more the individual who combines with outstanding intellectual urge of a personal nature the ability to co-operate with colleagues of equal attainments and ambitions; the obdurate lone worker can, in fact, impede the evolution by a group of scientific men of a joint integrated programme of research. The relation of master and disciple is the simplest form of internal organisation for research, and when it is successful it is one of the most salutary forms; and the Carnegie Institution is seeking to give it larger opportunity as younger men are brought in in greater numbers through its fellowship plans. Dr. Bush insists, however, that the central question is that of the way in which a group of scientific colleagues of equal stature plan in concert and interrelate their affairs, so that out of their combined thinking there comes a group or departmental programme which not merely meets the approval of a governing board but also affords a firm basis for the continuing effort of qualified minds.

The fundamental distinction between the conception and the process of research must also be kept in mind. At the outset, departmental research pro-

grammes are based upon the imaginative thinking of qualified individual minds. But, as Dr. Bush rightly observes, a collection of discrete projects aimed at individual objectives does not constitute a programme in the sense in which the term is used by an institution or a firm. Conceived as a synthesis or summary of joint effort planned for performance, a programme in this latter sense involves the choice of alternative methods and of sometimes widely separated objectives. Its preparation represents the first stage in the process, as distinct from the conception, of research.

Regarding, therefore, a well-prepared programme as something greater than the sum of its parts, Dr. Bush suggests that the most important single attribute of a scientific staff is the ability to perform as a group this function of estimating and balancing, integrating and interrelating. He reminds us that it is easy to overlook the fact that the organised joint effort which we know as research to-day, and which has so much expedited the maturing of science, is a relatively recent development. Indeed, in his analysis of the theory of the organisation of research, Dr. Bush simultaneously outlines the essential principles on which 'operational research' is based, and although he scarcely uses the word 'planning', his discussion provides a clear answer to the question how far it is possible for research to be planned.

This links up with some comments made by Sir Walter Moberly in his recent book "The Crisis in the University", where he quoted with approval Sir Henry Dale's words on the importance of the universities maintaining in Britain and elsewhere their full independence, and his emphasis on their function of promoting research with no immediate objectives but to widen the boundaries of knowledge. Like Dr. Bush, Sir Walter stresses the vital necessity of the academic values of a passion for truth, thoroughness in pursuing it to its end, a delicate precision in analysis, a judicial temper, a willingness to learn from all quarters, and an uncompromising insistence on freedom of utterance. No university can stand which does not uphold such basic values. Nevertheless, Sir Walter recognizes that a new balance must be found between teaching and research, and his remark that any teacher who tolerates intellectual dishonesty is out of place in the university gives the clue to the type of reform he would suggest. Entering his own protest against the tendency to attach too much weight to the number and bulk of published papers, Sir Walter challenges as roundly as Sir Charles Grant Robertson the practice of regarding all postgraduate work as research. It is not often that there is such public admission of the obvious truth that the neophyte can only rarely make original contributions of real value. Adulation of postgraduate research is apt to ignore the fact that in aspiring to add to the world's stock of knowledge the tyro in research may miss the more important objective of educating himself. Indeed, the search for some theme of research within his powers and in which he can make some contribution may, as Sir Walter remarks, lead the young investigator far from the centre to themes that are peripheral and often trivial; and it should be remembered that the great majority of those who take the bachelor's or master's degree by research are intending to pursue a career in research.

What Sir Walter says on this theme amounts to, in essence, a plea for more real thinking on the subject of research. A university needs to encourage and honour not only discoverers of facts but also explorers of ideas and appraisers of values. If it does this and can at the same time mark out those relatively few whose talents appear to fit them pre-eminently for a career of research, it will have done more to meet the needs of academic research, the research institutions and industrial research departments than by fostering large schools of directed research the output of published work of which is impressive only on paper. Speaking to the Central Council of the Association of University Teachers in Manchester on May 20, Mr. W. A. Wightman suggested, indeed, that the postgraduate side of a university department sometimes degenerates into a research factory organised solely for the advancement of knowledge in the chosen field, and manned by an ever-changing succession of workers whose business it is to do what they are told to do by the director. Under such conditions, the process of research, as defined by Dr. Bush, has not begun, nor can there be any fostering of the ability to co-operate which Dr. Bush rightly values. Mr. Wightman's statement was doubtless a deliberate exaggeration; but there is real danger in the position to which he directs attention. Early in the same week, again in Manchester, Sir John Stopford, in addressing a meeting of the University Court, also stressed the importance of ensuring that both teacher and student have leisure to think. At Leeds on May 20, an eloquent plea for the universities to play their part in the defence of the freedom to declare what we believe to be true, and to maintain it in speech and writing, came from Sir James Chadwick; and Dr. Mouat Jones also referred to the challenge of Sir Walter's book, although he took a less sombre view of the situation in the universities of Britain.

It is at least encouraging to find within one week university spokesmen in Manchester and Leeds recognizing so frankly that a firm grip upon the essential verities and values of humanity and civilization should be a central care of our universities, and our great need for understanding human nature. Sir John Stopford laid the main stress on the need for providing an effective means of bringing together the humanities and the sciences, and made the useful suggestion that, as new halls of residence come into being, the creation of junior residential fellowships might help to broaden the contacts and interests of students. Such aspirations, however, pre-suppose leisure for the university teacher or investigator and diminished pressure on the student, which involve further consideration of the question of numbers.

It must not be overlooked, however, that higher technological education is linked up with university education. Little progress appears to have been made in that field since, on May 27, 1946, the Lord President of the Council announced his acceptance of the general recommendations of the Barlow Committee on scientific man-power, which included endorsement of a recommendation of the Percy Committee regard-

ing the development of a limited number of technical colleges in Britain. Implementation by appropriate means of this common recommendation might well simultaneously ease pressure on the universities and make a valuable contribution to the professional training of graduates in research. If the increase of the student body is to come mainly, if not entirely, through the increase in university awards and scholarships, the question of quality becomes doubly important. Public expenditure on such a growing scale can only be justified when the training provided at the universities is such as to develop the first-class minds the nation needs, and to supply men and women who are capable of filling the responsibilities of leadership which a democratic society rightly expects of them, and also serving the community in those many capacities which demand character, ability and high standards of professional and technical skill.

Sir Walter Moberly has little to say on this matter in his book; he himself appears to lean to wider professional courses as providing the most hopeful line of advance, and he raises the issue whether professional training could be carried on as well anywhere else as in a university. This issue, of course, was frankly stated in the University Grants Committee's last report; but it requires much fuller discussion than it has yet received. Fresh impetus to its consideration may be derived from Mr. Winston Churchill's reference to technical education in his address on March 30 to the Massachusetts Institute of Technology.

ROCKET BALLISTICS

The Mathematical Theory of the Motion of Rotated and Unrotated Rockets

By R. A. Rankin. (*Philosophical Transactions of the Royal Society of London*, Series *A*: Mathematical and Physical Sciences. No. 837, Vol. 241.) Pp. 457–585. (London: Cambridge University Press, 1949.) 30*s*.

VERY little of a vast literature on rocket research has yet been published in book form or as papers in scientific journals. This is perhaps inevitable, since the development of the modern rocket has been due to the requirements of the War, and the numerous existing reports and papers, in many fields of scientific and technical endeavour, still carry a security ban. In recent years a book has been published, "The Mathematical Theory of Rocket Flight", by Rosser, Newton and Gross (for review, see *Nature*, 161, 3; 1948), which represents an American contribution and which deals with the ballistics of unrotated rockets. A paper by Dr. R. A. Rankin is the first extensive presentation of the wider theory including rotated rockets, and is by far the most important British paper so far published.

Dr. A. D. (now Sir Alwyn) Crow was entrusted in 1936 with the task of examining the possibilities of a rocket as a weapon of war, and under his direction at the Research Department, Woolwich, Mr. W. R. Cook and his staff laid the foundations of the theoretical ballistics. In a few years, working closely with such experimental results as became available, they had developed a two-dimensional theory which played an important part in the study of the behaviour of the early unrotated rockets which were later to prove their value as weapons both of offence and defence. A start was made in 1938 to establish a three-dimensional theory, and Mr. C. L. Barham with the systematic use of complex variables and vector methods extended the theory and obtained solutions which could be handled numerically. Many other mathematicians who were members of the original team, and of the larger group which was gathered together under Prof. L. Rosenhead early in the War, have all made their contributions to the present theory. Dr. Rankin in his paper pays gracious tribute to these other workers, including Mr. A. T. Wadley who, with Dr. Rankin, prepared an unpublished Ministry of Supply monograph on the subject; but to him personally must go the credit for the major part of the work on rotated rockets.

Rocket ballistics is concerned essentially with the behaviour of the rocket during the burning period of its propellant, and thereafter its motion is similar to the gun shell and can be treated accordingly. It becomes thus a matter of investigating the motion of a body which is losing mass continuously in the form of a high-speed gas jet and subject to external atmospheric and gravitational forces. If the rocket were perfectly symmetrical and the gases discharged uniformly, their resultant thrust were axial, and there were no disturbing forces such as wind, it is a relatively simple matter to establish the equations of motion and to integrate them numerically. Even so, extreme care must be taken in the derivation of the equations if errors are to be avoided, and if the principle of the conservation of momentum is to be correctly applied to the system of particles comprising the rocket and the escaping gases.

It is, however, the problem of the deviation of the rocket from its expected path that is of chief interest. Even in a group of rockets all of nominally the same dimensions, etc., there occur variations due to manufacture which lead to the very high dispersion of a salvo of rockets as compared to a group of gun shells. This dispersion, which is usually random in a set of rockets, has not yet been completely explained. It may be due to uneven gas-flow through the nozzle of the rocket, to distortion of components during the burning period, or to some other cause. It is, with experience, possible to predict fairly closely the dispersion of a rocket if it is similar to one of which the characteristics are known; but for a new rocket it is not so easy, even if due allowance is made for manufacturing tolerances. Similarly, the effect of wind or of initial launching conditions which cause a deviation has to be explained and predicted, which can usually be done.

The setting up of the complete equations in order to be able to consider asymmetries is in itself a major task, and this the author has done with commendable rigour and clarity. He next discusses the approximations which have to be made if the equations are to be integrated. These fall naturally into two classes: those which are sufficiently small to be neglected, and those which are assumed constant during the burning period. It is not possible in a paper of this nature to give all the numerical values that enable these assumptions to be made, but they can in most cases be readily justified. It must be realized, however, that the solutions obtained refer only to a class of rocket (solid propellant, high acceleration) for which these assumptions hold good, and do not

necessarily provide information on other rockets, for example, liquid-fuel rockets of low initial acceleration.

The first four sections of the paper are devoted to this initial work, and thereafter two sections deal with the solution of the equations when the spin of the rocket about its longitudinal axis is proportional to its forward velocity. Other forms of spin are later discussed, and there is a section in which the conditions necessary for rocket stability are proposed and in which there is a discussion of the type of instability that can occur if any of the conditions are violated. The remaining sections deal with the evaluation of the parameters and with a very useful discussion of the general character of the motion of a rocket.

The paper has appendixes containing tables of Fresnel and related functions required in the numerical evaluation of the deviations, and also numerical examples and figures showing the variation in deviation and yaw due to various effects.

Dr. Rankin has produced a paper which should become as well known to ballisticians as the paper by Fowler, Gallup, Lock and Richmond published in 1920 (*Phil. Trans. Roy. Soc.*, A, **221**, 295) on the motion of a spinning shell. R. C. KNIGHT

COLLECTIVE THINKING WITH COLLECTIVE MEMORY

Documentation

By Dr. S. C. Bradford. Pp. 156. (London: Crosby Lockwood and Son, Ltd., 1948.) 10*s.* 6*d.* net.

IT is good that Dr. S. C. Bradford lived to see published his cogent account of the history, principles, organisation, advantages and applications of the Universal Decimal Classification he so passionately believed in and helped so much to develop. It is convenient to find also a recapitulation of his pioneer work establishing the statistical law for the proportions in which articles relating to any particular subject are found distributed among periodicals of varying scope. If, however, "the art of documentation is to make available for mankind the sum total of the results of human work and thought", it was perhaps an instance of faulty classification to borrow the name of that very comprehensive art as the title under which to defend his opinion on the design of one of its tools. No one had a better right to an opinion than the author—*si monumentum requieris* as to the practical possibilities of the Universal Decimal Classification *circumspice* in his Science Museum Library—but alternatives exist which a balanced treatise would have to weigh.

The Universal Decimal Classification is an internationally standardized adaptation of the Dewey system of shelving books according to their main subjects. The class symbols which it provides for detailed items of knowledge are numbers in which each decimal place connotes a further particularization of the idea expressed by the one on its immediate left. If no book, article or abstract were to be printed without the appropriate decimal symbols already on it, subsequent bibliographical work would be reduced to mere sorting and filing.

Some of the factors which hinder full realization of this theory are criticisms of the Universal Decimal Classification as such, whereof the author himself lists and replies to no less than seventy. It may be significant that among those who prefer radically different systems of classification are found, in all countries, those concerned with patents, who for many decades have had a stronger urge to develop efficient means of documentary searching than any other body of technologists. Going further, there are some who question whether the filing of literature references in the sequence of symbols designed to pinpoint the logical position of each subject in a map of knowledge as a whole is fundamentally the best way to tackle the problem; for it is difficult to map a country the detail of which keeps altering and its frontiers expanding. Standardization of terminology, already proceeding in some directions and desirable on other grounds, would also help to remove some of the objections which Dr. Bradford urges against alphabetical indexing. Another alternative that he omits is the possibility of coding whatever is of potential interest in each reference in the form of patterns of holes in a card, of dots on a microfilm strip or of spots of magnetization in a steel band, so that, instead of having to file the references in a sequence, they can be in random order and run through a machine set to select and reproduce automatically any among them which have been coded in a particular way. Coding for this purpose is not the same thing as classification, and its philosophy needs to be worked out.

Surprisingly, the book has no index.

J. E. HOLMSTROM

CHAMBERS'S SIX-FIGURE MATHEMATICAL TABLES

Chambers's Six-Figure Mathematical Tables

By Dr. L. J. Comrie. Vol. 1: Logarithmic Values. Pp. xxii+576. Vol. 2: Natural Values. Pp. xxxvi+576. (Edinburgh and London: W. and R. Chambers, Ltd., 1948–1949.) 42*s.* net each.

CHAMBERS first published a volume of mathematical tables in 1844. This was enlarged and reset in 1878, and has been re-issued at intervals in the same format. The volume will be familiar to most people who have, at some time or other, needed to use 7-figure tables. It is now replaced by the volumes under review. They are in no sense a revision, but rather an entirely new publication. For good reasons given in the preface, six-figure values are given in the new tables instead of seven.

Volume 1 gives logarithmic values and is primarily intended for users who do not possess a calculating machine. The main tables are: logarithms of numbers, logarithms of circular and hyperbolic functions, and the logarithmic gamma function—the last is a short but important table. There are two separate tables of circular functions, one with the argument given in degrees, minutes and seconds, and the other with the argument given in degrees and decimals. This last table is made to serve also for angles expressed in radians by giving the tabular values of the argument in radians as well as in degrees, and by providing a special table for interpolation. There is a table of anti-logarithms; but the argument is given to four figures only, so that if anti-logarithms of six-figure numbers are required it is better to use the main table of logarithms inversely.

Volume 2 gives natural values, and the various auxiliary functions such as powers, roots and

reciprocals which are required by users of calculating machines. There are three separate tables of circular functions having the arguments given respectively in degrees and decimals, in radians, and in degrees, minutes and seconds. An unusual feature is the provision of tables of the inverse circular and hyperbolic functions; these occur so frequently in numerical calculation that the provision of direct easily interpolated tables will go a long way towards easing the computor's lot.

The other main tables in Volume 2 are: exponential and hyperbolic functions, natural logarithms, the gudermannian and its inverse, the gamma function, and various forms of the probability integral. There is also a table for converting from rectangular to polar co-ordinates. Minor tables which might be noted give factors of numbers up to 3,400 and primes up to 12,919. All the necessary auxiliary tables required for interpolation are given.

The explanations which are prefixed to both volumes are masterpieces of lucid style and offer much wise advice to the computor. The same applies to the annotated tables, given in volume 2, of formulæ for numerical integration and differentiation, and for the numerical solution of differential equations. The whole of Dr. Comrie's wide experience of numerical calculation has gone into the preparation and production of these volumes, and they are likely to become the standard tables for general use in work where more than four-figure accuracy is required.

M. V. WILKES

THE LABOURER AND HIS HIRE

The Technique of Industrial Labour Payment

Setting forth the Fundamental Principles of Time Study and Payment by Result. By C. L. Guest. Pp. xii + 198. (London: Macdonald and Evans, 1948.) 35*s*. net.

IN reviewing a book dealing with a narrow application of management technique such as time study, the reviewer is faced with a difficult choice. Should he judge the contents against the standard required to satisfy an experienced time-study engineer, or merely give a résumé of the subject-matter which may help students to decide whether the book contains subjects of interest to them? This book by Mr. C. L. Guest makes the choice even more difficult, for it may be of interest to both types of reader. I have therefore made a personal choice and reviewed the book against my own standard of what a book with this title should contain.

The book sets out the technique of operation-time measurement, outlines methods of payment by result, and details the routines required for the application and maintenance of incentive systems. The section dealing with time study is concerned primarily with the detailed technique, and describes stop watches, study boards, study sheets, and the working up of results. One chapter is devoted to labour and machine utilization and one to motion study. The second section of the book deals with the various methods of applying standard times to wages as a basis for payment by results, these methods being discussed and compared. The title of the last section, namely, "Applying and maintaining a payment by results system", is misleading, for it also covers subjects such as production planning and labour engagement, in addition to subjects related to the title.

The book adds little to the existing body of knowledge of time study or payment by results, and, in spite of the sub-title which states that it sets forth fundamental principles, seems to set forth only the detailed techniques which the author has used. While these seem perfectly sound, they are not principles and can be applied only in particular circumstances. For this reason, the book must be read with circumspection by beginners. The trained time-study engineer will already know the techniques set out and will gain little from the first and second sections. The third section will help time-study men who wish to gain knowledge of wage-calculation methods.

Many detailed examples are given to support the text, and results are presented graphically. These pages of detailed figures hinder rather than help the student in reading the book, and certainly do not assist in teaching principles. They may also irritate the reader who has to wade through this mass in order to get to the next part of the text, and as a result distract his attention from more useful data which follow. If these pages of figures had to be included, they would have been far better placed in an appendix where the reader with both time and patience could have read them at leisure. Graphical presentation of results of time studies and the use of mathematically correct curves is a dangerous habit, for the uncontrollable variables in work content make their validity problematical. In addition, this method may create the unfortunate impression that time study is an exact science by which the work output of human beings can be measured precisely.

The section dealing with methods of payment could well have been deleted, for every text-book on this subject in the last twenty years seems to have described the Halsey and Rowan methods and compared these with the straight piecework method. In addition, most industrial engineers agree that the Halsey and Rowan systems are outmoded and no longer capable of application. The only possible methods of payment by results which can be applied successfully are the allowed-time and straight piecework methods with a guaranteed minimum rate. Discussion of these other methods is then valueless except when studying the history of payment by results.

The author could well have devoted more space to effort rating, a subject which has not yet been adequately covered. It seems that he is also recommending the introduction of yet another standard for rating, this time with a base of forty. Surely there is enough confusion already caused by the use of two bases, the hundred and sixty, without introducing yet a third which has even less to recommend it. Again, surely the author does not seriously recommend the use of the three-watch study board shown on page 11. Labour relations are usually strained enough on the introduction of time study, without having them made worse by the use of such a formidable instrument.

This book failed to satisfy me, despite its wealth of detail and evidence of careful layout, because it is too involved for the student and too indefinite for the expert. It could be a useful addition to a selected list of text-books for managers and supervisors for reading, after the principles of labour payment had been established and examples of practical application were needed.

R. J. FRASER

SOVIET GENETICS: THE REAL ISSUE*

By Dr. JULIAN HUXLEY, F.R.S.

ONE misapprehension must be dealt with. Quite intelligent people often seem to think that, when a new theory wholly or partly replaces an old one, the whole fabric of the relevant branch of science has to be scrapped. That, of course, is quite incorrect. For example, the acceptance of relativity theory has not involved the jettisoning of classical Newtonian mechanics, but has in the great majority of cases only meant a slight correction, negligible for most purposes, in a number of laws. The body of ascertained knowledge remains: it has merely to be rendered more exact and incorporated into a more comprehensive framework. The same holds for neo-Mendelism: whatever new theories Lysenko or anyone else may contribute to genetics, the vast body of scientific fact comprised in neo-Mendelism remains as a contribution to our knowledge of the universe. (The difference between the two examples is that whereas Einstein has successfully introduced a radically new theory, or framework of ideas, to correlate the physical facts involved, there is at present no prospect of neo-Mendelian theory being overthrown by Lysenko's ideas—they are too vague to deserve the title of a framework—or, indeed, by any other general theory of genetics and evolution.)

Here I suppose I should turn aside for a moment to consider the nature of scientific law, for that august body, the U.S.S.R. Academy of Sciences itself, has utilized the subject as a weapon against the neo-Mendelians. In its manifesto that I have already cited, it asserts that "*Weismannite-Morganist idealist teaching is pseudo-scientific because it* is founded on the notion of the divine origin of the world and *assumes eternal and unalterable scientific laws*".

In the first place, even if both the statements were true, the term *pseudo-scientific* would still be quite unjustified. Plenty of good scientific work has been done by believers in a divine creation and by believers in scientific laws as something eternally given and merely waiting to be discovered. But, of course, the assertion applies nominally only to neo-Mendelian "teaching", although, as has been earlier made clear, the term "teaching" in this controversy is unjustifiably extended to include the entire system involved —facts, laws, hypotheses and theories, as well as underlying doctrine or philosophic tendency.

However, the statements simply are not true. I do not imagine that more than a handful of neo-Mendelian selectionists believe in the divine origin of the universe, and probably not one would believe in the divine origin of the world of living things as they exist to-day, whether through special creation or through a supernatural control of the process of evolution. This I know to be true of Morgan and Muller; and if I may take this occasion of affirming my own position, I have always believed (on the principle of William of Occam's razor) in the natural origin of living from non-living matter on this planet, and I consistently maintained, even through the sceptical period that ended around 1930, the view that Lamarckism was untrue, but that the evolution of life can be explained in terms of mutation and natural selection; and I am philosophically a non-theist. In any event, how can a belief in God or in creation affect the truth or falsity of scientific laws?

This brings me to the second statement. I am extremely puzzled to imagine what possible authority or evidence the Academy has for its assertion that neo-Mendelism ("Weismannite-Morganist tendency") assumes eternal and unalterable scientific laws. Just after I had taken my university degree, I used to go down occasionally to the John Innes Horticultural Institution to help Bateson count peas, and I can vouch for the fact that his and Punnett's formulation of the laws of coupling and repulsion, as they used then to be called, was not based on any metaphysical concept of scientific law. For them, as for myself or any other neo-Mendelian I can think of, the formulation of a law is an attempt to translate into general, and if possible simple and also quantitative terms, any regularities which they may discover in the phenomena they are investigating. Once a law is formulated, its validity (or otherwise) can be tested against new phenomena and its accuracy improved, or its shortcomings or its fallacy exposed. There is nothing eternal or unalterable about it. It is a shorthand way of stating that, in certain circumstances, certain results have been observed in the past and can be expected in the future. Mendel's second law, the law of independent assortment, for example, subsumes the following facts: when a cross occurs between animals or plants showing normal sexual reproduction and exhibiting normal chromosomal behaviour at meiosis, then, for characters dependent on genes not lodged in the same chromosome and not linked with the main gene or genes for sex-determination, we can in the F_2 generation expect a quantitative distribution approximating to that to be calculated on the assumption that the separation of any one chromosome-pair at meiosis has no effect on that of any other. But alter the circumstances, and the law will alter. If the characters affect viability, the proportions in F_2 will change from expectation; if two or more of the genes are in the same chromosome, they will not assort independently; if the meiotic process be upset, whether by tetraploidy, by directive non-random segregation of chromosomes, etc., the law will not hold. And, of course, the law could not have existed before sexually reproducing organisms with differentiated chromosomes had been evolved. Nor, in the view of most men of science, did it (as opposed to the phenomena which it subsumes) exist before it was actually formulated by the early Mendelians.

This brings me to another point concerning scientific laws. As work on a subject is pursued, the laws concerning it tend to appear less and less important, but become merged into a larger construction, a broad theory, or a vast system of phenomena all interrelated on the basis of certain simple and fundamental facts and conceptions. Thus what neo-Mendelians are concerned about is not whether the laws of segregation or of independent assortment are eternal or unalterable, but whether or not their labours have produced a large and important body of new truth. In this, large bodies of phenomena are elucidated

* Continued from page 942.

on the basis of the following elementary facts: (*a*) the existence of discrete material units of heredity lodged in a definite order in the chromosomes; (*b*) the constancy (complete self-reproduction) of these units save for occasional mutations (incomplete self-reproduction); (*c*) the differential viability and differential reproduction of organisms containing different contributions of these units. The great majority of the multifarious phenomena of heredity and evolution are now seen as consequential extensions, in various circumstances, of these basic factual principles.

This insistence that neo-Mendelism is wedded to the idea of eternal and unalterable laws is all the more strange, since one of Lysenko's main arguments against it (1948) is that its "so-called laws . . . are *based entirely on chance* [italics his], mutation, the separation of the *so-called paternal and maternal chromosomes* [italics mine] at meiosis, fertilization [and, he might have added, natural selection] being all matters of chance. Thus *living nature appears to the Morganists as a medley of fortuitous isolated phenomena without any necessary connection and subject to no laws.* Chance reigns supreme. . . . Unable to reveal the laws of living nature, the Morganists have to resort to the theory of probabilities, and since they fail to grasp the concrete content of biological processes, they reduce biological science to mere statistics. . . . With such a science it is impossible to plan to work towards a definite goal. Physics and chemistry have been rid of fortuities. That is why they have become exact sciences. *By ridding our science of Mendelism-Morganism we will expel fortuities from biological science.* We must firmly remember that *science is the enemy of chance*" [italics his].

Apart from the fact that the accusation of failing to grasp the concrete content of the subject seems singularly inapplicable to the latest discoveries of cytogenetics as regards the actual chemistry of the hereditary material, utterances like these reveal an extraordinary scientific illiteracy. Every elementary student is aware that the exactitude of many of the laws of physics and chemistry (such as the gas laws) depends on the fact that they are statistical laws—in other words, 'matters of chance'. When the physicists are no longer dealing with statistical laws, but are confronted with individual phenomena, regularity ceases, and chance (but now in the sense of individual accident) begins to reign. Lysenko is, of course, using the word *chance* in different senses and applying it to different scales of phenomena.

To return for a moment to the subject of 'eternal laws', it is possible that the Academy was not referring to what are usually called the laws of Mendelism, but to some supposed law of the non-inheritance of acquired characters. If so, this is again incorrect. In the first place, no such law has been formulated. Some neo-Darwinians have pointed out the logical difficulty of the inheritance of acquired characters in the commonly accepted sense of the word (modifications of the individual due solely to changed environment or to changed habits), and R. A. Fisher has mathematically demonstrated the impossibility of such inheritance, even if it did occur, accounting for the observed facts of evolution. But, more important, no neo-Mendelian of whom I am aware denies the possibility that the hereditary constitution might, in exceptional cases, be *directively* altered by agencies external to it—that is, not merely altered in a way unrelated to the agent as by all known mutagens, but altered relatedly, or even in some cases adaptively. One of the speakers in the debate expressed great surprise that a leading Morganist had so far gone against the principles ascribed to him by the Michurinites as to discuss the possibility of inducing related hereditary changes by means of antigens, as claimed by Guyer and Smith. I well remember the incident, since Carr-Saunders and I spent a great deal of time in repeating the work—unfortunately with entirely negative results. But no one believed in the existence of any eternal law which would prevent its being true.

As regards the third point—the taking over as evidence for Michurinite theory of results which are better explicable in Mendelian terms—I may cite the successful practical results quoted by various speakers (Lysenko himself, Yakushkin, Feiginson, Lukyanenko, Petrov, Krylov, etc.) as accruing in crop-plants from intervarietal hybridization followed by selection. This is always quoted against the neo-Mendelians as a Michurinite method. The fact is, of course, that in using this method (which in certain cases gives excellent results) the Michurinites are, without knowing it, simply utilizing the basic Mendelian principles of segregation and recombination, followed by Darwinian selection, but undiluted by any trace of Lamarckism*. (Luther Burbank, also a declared opponent of Mendelism, scored his successes by a very similar utilization of Mendelian phenomena.)

A similar comment may be made on the results proclaimed by various speakers (for example, Yudin, Greben, Minkevich, Shaumyan, Chekmenev, Feiginson, Ushakova, etc.). In general, they claim their results as a proof of Michurinism, or of the efficacy of Lysenko's methods of selection†. However, so far as I can understand from the summaries of their remarks, the results were achieved by straightforward selection, such as might be practised by any neo-Darwinian breeder. It is true that their successes are often contrasted with the 'failures' of the Russian Morgano-Mendelians; but these, in so far as truly failures, are presumably due to the latter having attempted to apply their principles in a faulty way. Especially in the early stages of modern genetics, the complexity of practical problems was not always grasped, nor the need for selection in relation to a particular environment. The great majority of neo-Mendelians to-day would agree with Lysenko's own formulation of the matter. He writes (1948): "Our science and practice of animal breeding . . . must be guided by the principle *to select and improve breeds in accordance with the conditions of feeding, maintenance and climate*" [italics his]. This, by itself, is impeccable from the neo-Mendelian and neo-Darwinian point of view, although his further condition, that one must "create conditions of feeding and maintenance most suitable to the given breeds", which in practice seems to mean merely providing a rich nutrition, is dictated by his Lamarckian beliefs, and could have no selective effect in the genetic sense.

Some other scientific aspects of the discussion may be briefly mentioned—the coining of the term *auto-*

* The wide crosses carried out by Michurin himself were made for the most part with fruit-trees. In these, owing to differences in chromosome-number and to the existence of self-sterility and incompatibility mechanisms, the hybrid is very rarely fertile. This does not prevent the Lysenkoites from claiming that the same mysterious 'principle', according to which a wide cross 'shatters' the heredity which can then 'assimilate' new environmental conditions, operates both in these and in fully fertile crosses.

† Mr. Richens tells me that the Russian word *selekciia* means breeding in general. It is impossible to judge from the reports whether speakers were not sometimes using "selection" in this sense.

genesis, unknown or at least unused, so far as I am aware, in the West, to denote what Lysenko thinks to be the belief of his opponents as to the determination of evolution entirely by intrinsic factors. As a matter of fact, almost all neo-Darwinians believe firmly in the guiding activity of the environment, mediated through natural selection, so that the "creative" aspect of evolution in producing novelty is due not only to the new bricks of the building provided by the "autogenetic" process of mutation, but also to the directives furnished by the inorganic and biological environment to the moulding force of selection. Further, of course, neo-Mendelism explicitly states that all phenotypic characters originate through the interaction of genes and environment during ontogeny.

In general, the controversial methods of Lysenko and his followers are unscientific, in that their attacks are almost always directed against the youthful and sometimes crude neo-Mendelism of twenty to forty years ago, when it was groping towards new facts and more comprehensive formulations, and that they have either deliberately neglected its remarkable modern developments, or have not taken the trouble to understand them (see Stern, 1949).

The rejection of the concept of pure lines, in spite of its experimental verification and theoretical validation, may be due merely to wish-fulfilment, or more probably to the fact that what the Lysenkoites have called pure lines in the U.S.S.R. are not really such at all, and therefore *are* capable of modification through selection; but it can certainly not be justified as a scientific procedure. Nor can the condemnation of inbreeding as a practical method be justified. It is even more difficult to suggest a motive for this, especially as it has been applied so very successfully in the United States to maize, and is now beginning to be applied (for example, by Henry Wallace) to fowls. Yet Serebrovsky is criticized by Petrov for advocating inbreeding in poultry.

I would also like to cite a statement which I find scientifically and, indeed in every respect, incomprehensible. Yudin is summarized as saying that 'the principle of vegetative hybridization is important in animal husbandry' (ref. 1; p. 654)*.

But I must now pass to Lysenko himself, for his personal role has been of unusual importance. I will not attempt to deal with the details of his theory of heredity and evolution, as that has been discussed elsewhere (for example, by Hudson and Richens, 1946; Ashby, 1947; Darlington, 1947). The fact is relevant, however, that it is an essentially naïve and *simpliste* theory, based on analogy rather than on induction. In his own words (1948): "*heredity is the effect of the concentration of the action of external conditions assimilated by the organism in a series of preceding generations*" [italics his]. Heredity is "inherent in any particle of the living body". Yet (though in general he denies any special genetic role whatever to the chromosomes) in one place he admits that heredity is "transmitted through the chromosomes in the sexual process", without seeing that, the chromosomes being what they are, namely, the vehicles of a row of spatially or chemically differentiated unit-sectors or genes, this admission has as its direct consequence the various laws of neo-Mendelian genetics. He speaks contemptuously of Mendel's laws as "the pea laws", and in the 1939 discussion on genetics[a] referred to the 3 : 1 ratio as "the work of a devil". However, though he must accept the evidence of his senses through the microscope as to the existence of chromosomes, he refuses to admit genes. He himself simply ignores them; but his philosophical helper, Prezent, as noted elsewhere, categorically denies their existence.

It should be noted that this admission of the existence of chromosomes is the only common element between Lysenko and the neo-Mendelians. Otherwise he prefers to throw everything overboard and start again from scratch—assuredly not a very advanced scientific procedure.

I said that it is relevant that his theory is naïve in type, for Lysenko would be the first to repudiate any claim to be an intellectual. He is a peasant, with a passion for practical results in his work. He is also an ambitious man. He has not had any advanced scientific education, but has come to science from practical agriculture. He is politically a zealous communist, and also a powerful one, having been for a period a Vice-President of the Supreme Soviet. A Russian scientist described him to me as the Rasputin of Russian biology; but after listening to him and talking with him, I am sure that this is wrong: Savonarola would fit better, though by no means perfectly. He is, I am sure, sincere; but he is certainly fanatical and I should imagine ruthless.

I would like to put on record one incident which I personally witnessed. During the Academy celebrations in 1945, I had asked to see Lysenko and his results, but had been told that he was too busy. However, after repeated requests, it was suddenly announced that he would lecture next day, and I went to listen, accompanied by Prof. Ashby, and by an excellent interpreter who was also a biologist. Her running translation was at one moment drowned by a burst of laughter from the audience. On my asking her afterwards what had provoked this, it appeared that Lysenko was discussing Mendelian dominance and segregation, which his opponents sometimes brought up against him. Dominance, he said, was easy to explain on his own theories; it was the 'assimilation' (or perhaps 'digestion'—I forget which) of one heredity by a second, after a cross. But segregation (of recessive characters in F_2)? That also was easy. 'We know in our own persons,' he said, 'that assimilation (or digestion) is not always complete. When that is so, what happens? We belch. *Segregation is Nature's belching*; unassimilated hereditary material is belched out' (presumably in a 1 : 3 ratio!). I cite this remark as further evidence of the fact that, scientifically, Lysenko can only be described as illiterate. I use the word as meaning that it is impossible to discuss matters with him on a scientific basis, as appeared in Ashby's and my subsequent conversations with him, and is evident throughout his remarks at the session. Sometimes he appears ignorant of the scientific facts and principles involved, sometimes he misunderstands them, sometimes he distorts them, sometimes he counters them with bare assertions of his own beliefs.

I am glad to be confirmed in this by Dr. Harland, one of the world's most eminent men in the fields both of pure and applied genetics, who in a letter to *Discovery* (1949) writes as follows: "In 1933 . . . I

* I can now quote his verbatim remarks (ref. 3; p. 412). He says that the arguments advanced by Zavadovsky "to prove that it is impossible to apply the principles of vegetative hybridization to animal breeding are to my mind groundless and were prompted by his narrow conception of the problem. *Vegetative hybridization must not be reduced to the mere grafting of tissues.* A broader view of vegetative hybridization must be taken. *Properly speaking*, it is the influence of the soma upon future generations." So you can have "vegetative hybridization" without hybridization! This is only one among many examples of the looseness of thought and the misuse of scientific terms which enable the Michurinites to fit almost anything into their 'doctrine'.

saw Lysenko in Odessa, catechized him for several hours and inspected his practical work. It was quite clear that Lysenko was blazingly ignorant of the elementary principles of both plant physiology and genetics. . . . You simply couldn't talk to Lysenko —it was like discussing the differential calculus with a man who did not know his 12-times table. When I say that some of his assistants were using plant pots without drainage holes, you amateur gardening readers will understand.

"After much persuasion Lysenko gave me a formula for vernalizing cotton seed. After giving the method a full trial, we found that it just didn't work. . . ." (Ashby informs me that, while in the U.S.S.R. in 1945, he ascertained that vernalization of cereals was not being employed practically, apparently because Lysenko's method, though satisfactory in laboratory conditions, could not be satisfactorily adapted to large-scale practice.)

My interpretation of Lysenko's role in the matter is as follows. He genuinely believes that he has obtained the inheritance of acquired characters by certain special treatments, and that he has been successful in effecting vegetative hybridization by means of grafting. Neither of these results would be possible on the basis of neo-Mendelian fact and theory in their present form. On the other hand, as already pointed out, the results might have been obtained owing to the non-observance of certain procedural precautions in the crucial experiments.

Lysenko, together with his non-scientific backers in the Communist Party, in my opinion, has not understood the solidity of neo-Mendelism as a system or a branch of science, how massive and yet how complex it is, how well grounded in fact and how well checked by mathematical deduction. He has only seen that its proponents are hindering his work, in which he genuinely believes. Sooner than have his experiments repeated with new and adequate precautions, sooner than take the trouble to try to reconcile the two 'opposites' involved; above all, sooner than admit that he may have been mistaken, and that living Nature is not so easily taken by storm, he has made up his mind to treat neo-Mendelism as the enemy, and to root it out from the U.S.S.R., so as to leave the field free for his own ideas. And because politics and politico-social philosophy in the U.S.S.R. can be used to control science and scientific movements, and because he happens to be politically powerful, he has succeeded.

Meanwhile, however, it seems most likely that his political power was due to backing by the strong anti-Mendelian element in Russian political circles, who deliberately selected him as their scientific figure-head and agent. I cannot here discuss the various possible reasons for this strong anti-Mendelian and pro-Lamarckian bias among the political and ideological leaders. It is, however, evidenced by many historical facts (I rely on Muller (1948), Goldschmidt (1949), Dobzhansky (1949), and unpublished information from Muller and Ashby, besides my own knowledge).

Already in the early 1920's, Dobzhansky was advised by his professor against a career in genetics; in 1929, the notorious Kammerer was being glorified in a film by Lunacharsky (Goldschmidt, 1949); between 1933 and 1936, several neo-Mendelian geneticists were banished, sent to labour camps, or imprisoned (Muller, 1948); in 1936, after violent attacks in *Pravda*, the splendid Medicogenetical Institute under Levit was dissolved (Muller, 1948); Muller, when in the U.S.S.R., ascertained that Stalin believed firmly in the doctrine of the inheritance of acquired characters.

In 1935, the exaltation of Lysenko began. In 1936, the Seventh International Congress of Genetics, due to have been held in Moscow in 1937, was called off because the Party did not wish evolution and human genetics to be publicly discussed in the U.S.S.R. (Muller, 1948, and unpublished). In the same year, however, a purely Russian conference on genetics was staged, in which the public attack on neo-Mendelism was first deployed. The proceedings of the two later similar conferences, in 1939 and 1948, show how the attack was later intensified, and how it became progressively less scientific and more ideological in character. Only after the 1939 conference could Vavilov be accused of sabotage—because he had stated that it might take at least five years to construct certain improved varieties of wheat! (Muller, unpublished).

Finally, in 1948, we have the adoption of Michurinism and the banning of neo-Mendelism as part of the party line of the all-powerful Communist Party.

Lysenko was an eminently suitable man to lead the attack in public, and he was selected to do so. But it seems clear that the major responsibility for the banning of neo-Mendelian genetics in the U.S.S.R. rests on the political and ideological leaders (one cannot disentangle the two aspects) in the Communist Party.

* * * *

I have tried to present in some detail the situation concerning genetic science in the U.S.S.R. This must be related to a more general picture—the situation of thought and creative expression under Communism —though I can here only touch on its broad lines. In what follows, I shall use 'thought' as a convenient brief general term, to cover not only philosophy, but also creative expression in letters, art and music.

In the U.S.S.R., as is now common knowledge, thought in this extended sense has been to a greater or lesser extent compulsorily socialized—subordinated to an over-riding social philosophy and subjected to State (political) control, so that its freedom or autonomy is consciously and expressly restricted. It is, of course, obvious that thought is nowhere completely autonomous, being always limited by its material, social, and spiritual or intellectual environment; but this limitation is, in the Western world of to-day, for the most part an automatic conditioning, not a conscious restriction, and is moderate in extent.

The restriction of thought and expression in the U.S.S.R. operates rather differently in different fields. In politics it of course operates through the one-party system, which allows freedom of political thought or expression only within the limits of communist party doctrine.

In history and the social sciences, restriction is as severe, and perhaps even more productive of distortion (as an example of the distortion of history, I may cite the fact that in the Museum of the Revolution there is not—or was not in 1945 when I was in Moscow—any mention of the part played by Trotsky in the Revolution).

In philosophy, it appears to operate by reference to a tradition of authority and orthodoxy. To read the recent discussions on philosophy in the U.S.S.R. (summarized in *Europe* two years ago) is rather like being transported back to one of the Councils of early Christianity, except that the authorities with whom one must conform are not the Fathers of

the Church, but Marx, Engels, Lenin and Stalin. The function of philosophy in the U.S.S.R. is not to explore the bases of human thought and action in general, but to clarify and develop a particular philosophy, that of neo-Marxism (as we may call Marxism as brought up to date since 1917), which provides the theoretical basis for political activity. In the arts, on the other hand, the positive criterion to which they must conform is "socialist realism", as opposed to the negative criterion of "formalism". These criteria are interpreted rather differently in different arts—in music, for example, a good deal of subtlety has to be employed to give a sense to the term *realism*; but rather crudely it may be said that "socialist realism" is intended as the justification of the belief that the arts should be easily intelligible to every citizen, and should have as their only, or at least their prime, function the social one of providing emotional outlet, focus and drive for the activities of society in war and peace, as against that of new exploration or of expression for the individual artist, or of private enjoyment by the individual citizen*.

In natural science, it is too much to suggest that everything shall be readily intelligible to everybody. On the other hand, there is apparently in many fields, and perhaps notably in agriculture, a tendency to stress the practical aspects of science in providing control over Nature, as against the 'pure' aspect, as providing knowledge and understanding of Nature. It is therefore sought to associate the practical workers in applied science as closely as possible with research; and to achieve this it is desirable that scientific theories should be of as simple a nature as possible. Elaborate and unfamiliar theoretical constructions (such as that of neo-Mendelism) cannot be expected to appeal to the practical man who is anxious for results and likes to feel he understands the great adventure in which he is participating. They can therefore be conveniently discouraged by being branded as formalist (as well as being stigmatized as politically undesirable if that too is required).

The attack on the theory of probability is another example of the appeal to naïve practicability. Soviet science does not want mere probability in its theories, because it wants certainty of results: this is in no sense an unfair travesty of the position taken by Lysenko himself—it would presumably be over-complicated and difficult to explain that only by means of the theory of probability is it possible to evaluate whatever degree of certainty a scientific result may have.

In the particular case of genetics, it would seem that Lysenko's theories have a greater appeal to the practical man, because of their *simpliste* nature, in equating the highly complex processes of heredity to the apparently simpler and, at any rate, more familiar ones of digestion and assimilation, and in their naïve view that environment acts directly upon heredity to produce adaptation, instead of indirectly *via* the mechanism of selection; and further, in using Michurin's homely ideas, such as the 'shaking' or 'shattering' of the 'heredity', instead of trying to analyse what really happens in the complex biological entities and processes that are actually involved.

It should in all fairness be noted that many branches of Russian science have not been treated in the same fashion as genetics. There appears to be no specifically Marxian ideology, still less any party line, prescribed for chemistry or biochemistry, for mathematics (apart from probability theory), for geology, palæontology, ecology, taxonomy, plant physiology, etc. This is presumably because their pursuit has not yet raised any issues of ideological importance.

Whatever the reasons, the "socialization" of biological science in the U.S.S.R. has proceeded along the lines I have indicated—appeal to immediate utility, to the partial or total exclusion of the appeal to the discovery of new facts and new truth; appeal to national patriotism and class sentiment, so that science is regarded primarily as an instrument of the class struggle and its national extensions; the subordination of scientific to philosophical theory, and of scientific activity to an over-riding socio-political point of view; and finally, the appeal to authority, in the shape of a party line, in regard to scientific research and education. In passing, it must be noted that a great deal of what may be called the philosophical labelling of tendencies in science has been exceedingly arbitrary, and often, in my opinion, actually erroneous. Thus, as already mentioned, it is a perversion of terms to call neo-Mendelism *idealist* and *anti-materialist* when its chief merit has been the discovery of the material basis of inheritance. As regards the name-calling of individual geneticists, I knew Morgan intimately, and know that it is absurd to impute any philosophical or political motives to him; and Muller, who is stigmatized a 'bourgeois' or 'reactionary', was actually in difficulties in the United States for some years of his most fruitful period because of his left-wing and pro-Russian attitude. In any case, a fact is a fact whether discovered by a communist or a fascist, whether in the United States or in the U.S.S.R.

In any event, science in the U.S.S.R., together with other fields of intellectual thought and creative expression, has now become in principle an activity to be exercised in subordination to an over-riding doctrine. This doctrine is doubtless in part a deliberate rationalization of the political practice and aims of the rulers of the U.S.S.R., but none the less for that, constitutes an extremely powerful driving force for the Soviet State and Soviet society. According to this doctrine, the developing socialist society of the U.S.S.R. in particular, and of communist countries in general, finds itself involved in a desperate and inevitable struggle with capitalist society in the other major countries of the world. Not only is that struggle inevitable, but it admits of no compromise: according to the official philosophy of Soviet communism, it must continue until the victory of communism is assured.

Science accordingly comes to be regarded as an organ of the developing socialist society and therefore as one of its weapons in its struggle against the rest of the world. Furthermore, the socio-political struggle is transferred into science, which is then seen as divided into two camps, as inevitably and as irreconcilably opposed as, in the view of orthodox Soviet political philosophy, are communism and capitalism, the communist and the bourgeois or capitalist type of society.

It has puzzled many observers to note that in the genetics controversy the official Soviet scientists have abandoned one element in orthodox Marxism, namely, the principle that advance is effected through the reconciliation of opposites, by the reconciliation of thesis and antithesis in a higher synthesis. However,

* Since this was first written, the situation has been described with special reference to music in Alexander Werth's "Musical Uproar in Moscow", and is to be treated in a more general way by John Langdon-Davies in his forthcoming "Russia Puts the Clock Back".

the explanation is, I think, the simple one I have just advanced, namely, that the scientific controversy has been subordinated to and indeed made a part of the class struggle, and so has come to partake of the irreconcilability which the Marxists have always pronounced to be a feature of the more general socio-political conflict*.

One further consequence of this state of affairs is the injection of patriotism and xenophobia into science, as we have already cited. For some little time past, rebukes have been administered to Soviet scientific workers for servility to foreign or bourgeois scientific theories, and the principle of secrecy in science has been extended further in the U.S.S.R. than elsewhere, since Soviet scientists in general, and not only those engaged on war research, have been warned not to speak freely to foreign scientists about scientific discoveries in the U.S.S.R.

Once this over-riding system of ideological criteria is set up for science, it becomes all too easy for men of science who enjoy political power, or are in a position of authority, to use it for the discomfiture of their scientific opponents; and, in fact, in reading the summary of the discussion on Soviet biology, one cannot escape the conclusion that Lysenko and his followers have thus taken advantage of the situation. Because a system of authority and orthodoxy exists in the U.S.S.R., and because within such a system certain philosophic labels connote blame and condemnation, those labels, it would seem, have often been attached with the deliberate purpose of administering a thorough beating to one party to a scientific dispute.

The idea of subordinating scientific activity to ideological and political considerations is at least understandable in a society such as the U.S.S.R. What is difficult to understand is how, in the case of genetics, this subordination has been pushed so far as to deny the validity of well-established facts and concepts, and to proscribe an entire branch of science, while giving official approval to an alternative system which is demonstrably inadequate from the point of view of scientific method, and the facts and concepts of which have not been properly established.

Russian science in general stands at a high level. How can the Academy have given its blessing to Michurinism, whose facts are notoriously and obviously dubious and whose crude interpretative principles bear no resemblance to a scientific theory in the accepted sense? And how can it have pronounced the scientific condemnation of Mendelian genetics, seeing that in its fifty years of existence it has steadily developed until in all other countries it is accepted as one of the most vigorous and successful branches of science?

Equally puzzling, how can the political authorities have given official sanction and exclusive encouragement to Michurinism, when impartial scientific advice, if given without any political pressure, would have told them that its methods and ideas, when not definitely false, are inadequate and unscientific, in that they do not meet normal scientific criteria? Do they not know that bad science cannot produce good practical results?

Clearly, no one outside Russia can answer such questions. But one may hazard a guess. My guess is that many factors have been involved. As regards practice, Lysenko and his school are continuing to obtain results, partly through improvements in agronomy which have nothing to do with genetics; partly through genetic methods which, although claimed as Michurinist, are in reality based on Mendelism; and partly through the enthusiasm which Lysenko has been able to inspire among agriculturists. So the practical bankruptcy of Michurinism is for a time disguised. The political authorities appear unable to distinguish between scientific theories in the proper sense of the word, and vague interpretative hypotheses which are so flexible that they can be adjusted *post hoc* to account for a great deal that is really better explained on quite other principles. So the theoretically expected crash may be long postponed.

Then Marxism, since the Revolution, has always had pro-Lamarckian and anti-Mendelian leanings. This is partly because Lamarckism promises shortcuts both to agricultural and social improvements, while Mendelism cannot do so, and in its earlier stages was not of much service in many practical fields; partly because Mendelism has an anti-egalitarian element, the implications of which have been much exaggerated in the U.S.S.R. as a stick with which to beat eugenists and racialists; partly because the direct effect of environment and heredity postulated by Lamarckism seems at first sight much more "natural" as well as simpler than the indirect effect, *via* the natural selection of mutations, which Western genetics has demonstrated, but which Lysenko dismisses as "unthinkable".

A further factor would seem to have been the rise of a patriotic nationalism in the U.S.S.R., which has affected science as well as all other fields. Michurin was one of the few Russians who could be selected as having made distinctive contributions to agricultural improvement. He had been favoured by Lenin; his views not only fitted in with Lysenko's, but with the Lamarckian bias I have just mentioned. He could be, and was, made one of the chief symbols of a distinctively Soviet genetics, and once this was done, neo-Mendelism, being irreconcilable with Michurinism, could be branded as bourgeois, foreign and unpatriotic.

Finally, there was the fact of Lysenko, an admirable figurehead for the anti-Mendelian faction in the Communist party, and quick to turn all these other factors to account in his own interest.

As for the Academy of Sciences, I do not see how its action can be explained, except as a result of the subordination of science to politics and ideology. Once a party line in biology had been laid down, the Academy had to toe it like everybody else. In the meetings of the Academy in August 1948 (as reported in *Izvestia*; and see Nujdin[3]; p. 120), it was revealed that up to 1945–46 the Academy had been seriously considering resolving the genetics controversy by setting up a special Institute to deal with Mendelian genetics, under Dubinin, so that both parties in the dispute could have continued working on an equal footing. The only significant event since then was that Michurinism was officially approved and Mendelism officially disapproved.

However, none of these circumstances really extenuates the error or crime (in science the two can often be equated) of repudiating scientific method and rejecting the appeal to fact, and, in so doing, of repudiating the unity and the international character of science. All that we can say is that they help us to understand what has happened.

* The same is true for culture. At the Wroclaw Congress of Intellectuals last summer, several Soviet speakers, including Ilya Ehrenberg, were strong in denunciation of Western or bourgeois culture and ideology, and emphatic in asserting that there could be no reconciliation between them and Soviet culture and ideology—or rather no compromise, since this avoids the use of the ideologically awkward term *reconciliation*.

It is, of course, true that the freedom and autonomy of science have been infringed upon in countries other than the U.S.S.R. The total nature of modern war is such that secrecy is imposed on all men of science carrying on research for war purposes, even in peace-time. This, however, affects only a fraction of scientific work. What is in dispute is merely the limit of the 'secret sector'; and men of science are still free to devote themselves to work of a non-military nature. Freedom of publication is also limited in certain branches of industrial research; but here again science as a whole is not involved.

Again, in some countries, such as the Argentine, many university scientists have been dismissed for political reasons. But even in such cases politics does not presume to dictate the *scientific* admissibility of theories or branches of science.

The nearest approach in the Western world to ideological control of science was the legislation prohibiting the teaching of evolution in Tennessee and some other States of the U.S.A. But even this was partial in the sense that it affected only a few States, and only the public institutions in those States.

So far as I am aware, in modern times it is only in the U.S.S.R.* (and, though to a somewhat lesser extent, in Germany under Hitler) that science has lost its inherent intellectual autonomy, in the sense that the admissibility of its theories, laws and facts is judged not on their scientific merits but in relation to political and philosophical doctrines, and research and scientific thought are subordinated to the directives of a political party.

As a direct consequence of this, science as a whole has lost its unity. It is no longer in essentials a world activity, that is, one transcending the partial frameworks of nationalism and religion, but has become split into two. The Nazis tried to split it into German, Aryan or Nordic science as opposed to non-Aryan, Jewish or Bolshevik science; the Russians have now succeeded in splitting it into Soviet, Marxist, Communist or materialist science as against foreign, bourgeois, capitalist or idealist science.

Nazi Germany paid for its attacks on scientific autonomy and unity by a deterioration in the quality of its scientific work. The U.S.S.R. will doubtless in due time pay an equally heavy price†. But this can provide no satisfaction, except perhaps to the minority whose hostility to the U.S.S.R. over-rides all other considerations. All men of science worthy of the name, all who really believe in the possibility of progress for the human species as a whole, and in science as an indispensable agency for securing that progress, all who believe that the search for new truth is one of the highest activities of man, must feel acute regret at the action of the U.S.S.R. through its Academy of Sciences.

But regret is barren. We ask immediately whether there is nothing to be done. In the first place, we must realize that the action of the U.S.S.R. is only an extreme and exaggerated manifestation of a general situation. The general situation is constituted by the familiar trend towards a greater centralization and a greater organisation of society. This again is regretted by some; but it would appear to be inevitable in the present stage of the world's history, and many consider it to be a necessary prerequisite not only for the greater efficiency of the social organism but also in the long run for the greater happiness and fuller development of individuals. There are, however, good and bad ways, or at least more desirable and less desirable ways, in which this trend can be realized. This applies both to broad social and economic organisation, and also to the way in which science is to be integrated with the rest of the life of society—for clearly science cannot escape the operation of the general trend.

Our question—whether there is nothing to be done—now resolves itself into three more particular questions. What can men of science do to see that the general trend towards the integration of society develops in the best possible way? What can men of science do to see that the integration of science with other social activities does not infringe on its autonomy and its unity? And what can men of science do to modify the policy of the U.S.S.R. in subordinating science to philosophical and political orthodoxy?

Although the first question is perhaps the most important, it is difficult to find an answer to it, and especially a generally agreed answer, and I do not propose to do more than touch upon the matter. I personally would suggest something of this sort. It is of great importance for a society to possess some kind of ideological driving force. National patriotism may suffice in times of war, but not in peace. When religious belief is strong, it may provide the ideological drive; but this is assuredly not the case in the Western world to-day, where religion is not only fragmented into many churches and sects, but also no longer provides a dominant appeal to the majority of people. In the U.S.S.R. and other communist countries, on the other hand, communism does provide such an appeal, and an appeal both theoretical and practical in nature*. To provide an equally powerful and equally general appeal, I believe that only some kind of dynamic or evolutionary humanism will suffice, a belief that man has the duty of carrying the general process of evolution to new heights, and that in discharging that duty rightly he will be providing and ever expanding new possibilities of fuller living for future generations. If so, then this evolutionary humanism must be based on science, and it will be the task of the men of science not only to provide the material basis for the heightened standards of living, but also the theoretical and philosophic background for the new ideology—what for a religion would be its theological framework.

Bernard Shaw in a recent article (1949) supports Lysenko and the action of the U.S.S.R. Academy of Sciences on the ground that neo-Darwinism is fatalism or determinism and that "it becomes plain

* While this article was being written, the campaign was being extended to other countries within the Russian sphere of influence (see, for example, *Herald Tribune*, Paris, Feb. 22, 1949). In the universities of the Russian Zone in Germany, the professors in biological subjects have been requested to introduce Lysenko's teaching; in Czechoslovakia an article in the chief organ of the Communist Party extols Lysenko and rebukes "conservative scientists in the West who try to stop what cannot be stopped"; the Bulgarian Communist Party has demanded the repudiation of the "anti-Marxist biological views of Mendel, Weismann, and Morgan", etc. Western scientists in other fields have also been attacked. Thus the Academy of Sciences has denounced Einstein, Bohr and Heisenberg as "obscurantists" or "bourgeois metaphysicians".

† Russian science has already begun to suffer. In genetics and related fields, such notable workers as Vavilov, Levit, Ivanov and Karpechenko have lost their positions in the past, to be followed now by men like Dubinin, Serebrovsky, Gause and Schmalhausen. The ideological atmosphere was such that H. J. Muller relinquished his post after a few years, and excellent Russian geneticists like Dobzhansky and Ephrussi preferred to work in other countries.

* Even when the appeal is mainly to the party élite, it is still capable of being used as the theoretical or propaganda background for action or even compulsion, much as theology could be used in the past, in relation, for example, to intolerance or religious persecution.

at once that it is a doctrine that no State can tolerate, least of all a Socialist State, in which every citizen shall aim at altering circumstance for the better purposely and conscientiously, and no criminal militant reactionary can be excused on the ground that his actions are not his own. . . ." Later he states that "the real issue is between the claim of the scientific profession to be exempted from all legal restraint in the pursuit of knowledge, and the duty of the State to control it in the general interest as it controls all other pursuits".

Mr. Shaw here reveals his customary incomprehension of the nature and methods of science (I heard him once at a luncheon girding at scientists: "they tell us that the sun is 90 million miles away. How do they know? They haven't been there".). Even if neo-Darwinism did necessarily or usually encourage a fatalistic philosophy, which it does not, this would not mean that it was untrue, or even that the State should forbid it to teachers or research workers. Nor does Lamarckism become fact because Mr. Shaw and the U.S.S.R. Academy of Sciences feel that it would be nice if it were true.

He confuses the issue over the question of legal restraint, for in regard to this he adduces the right of the State to prevent a man of science from boiling his mother "to ascertain at what temperature a mature woman will die". Of course, the State has the right to limit and regulate research (as, for example, by means of laws and regulations concerning vivisection); but what is here at stake is something quite different—the right of the State to reject scientifically ascertained facts and to brand whole branches of science as false or not permissible.

The second question is more specific and more immediate. I may perhaps re-phrase it thus: How should men of science act in the face of the increasing concern of the State with science, and the consequent increasing pressure of the State on science?

Can they accept the existence of an official scientific policy? Can they accept the possibility that the majority of men of science shall be paid by the State and that the major cost of scientific work shall be borne on Government funds? Can they accept official direction as to what subjects shall be investigated?

I think that they can (indeed, that they must)—but with certain clearly formulated provisos. A Government is at perfect liberty to embark on a large-scale and comprehensive official scientific policy. It can legitimately decide that that policy shall be predominantly practical—designed to raise the standard of life, to improve health, to increase production, or to promote military efficiency. It can legitimately demand that the scientific curriculum throughout all stages of education should be adequate and should be framed so as to give the best possible understanding of Nature and man's place in Nature, of the social functions of science and of its intellectual and practical importance. It can legitimately insist on large-scale educational campaigns outside the school and university system to help the general population to understand the value and importance of science as a whole or of this or that branch of scientific work, or to make them feel that they are actively and intelligently participating in the nation's scientific effort. It can legitimately do everything in its power to check superstition, to combat unscientific or anti-scientific attitudes of mind, and to promote an understanding of scientific method, and of its value and importance.

Probably all men of science would agree that it is legitimate, and most of them that it is desirable, for a Government to embark on such a policy. But they would assuredly only agree on certain conditions. In the first place, they would say a Government* has no right to pronounce in any way on the truth or falsity of any scientific facts, laws or theories, nor to exert pressure in favour of their acceptance or rejection by scientists. It must not subordinate the intellectual autonomy of science to any other criteria, whether religious, philosophical, or political, nor seek to impose upon scientific truth standards other than its own, nor relate scientific activity to any orthodoxy or authoritarian principle, nor, most of all, impose a scientific orthodoxy.

As implication of this, it must consult scientific opinion in forming its scientific policy, and leave all essentially scientific decisions in the hands of men of science. On the educational side of its scientific policy, it must, of course, consult educationists as well as scientists, and recognize their autonomy in their own sphere.

It must recognize the special characteristics of science and the scientific method—the fact that it is essentially a universal activity; that for its advance it depends very largely on freedom of publication, which in its turn implies freedom for other scientific workers to test and re-test published conclusions; that major advances in scientific knowledge cannot be planned to order, and that new possibilities of practical advance often derive from the most unexpected quarters, including investigations undertaken with no practical aim.

As a consequence of these characteristics of science, the State should permit the utmost freedom of publication consonant with military security; it should encourage the international exchange of publications and research workers to the fullest extent; and it should not insist on all research, even all research paid for out of Government funds, being directed to immediate practical objectives, but should leave a considerable "unplanned sector" of fundamental research to the free choice of the pure scientist.

In education, while not in any way minimizing the importance of science as an organised body of tested knowledge, it should also recognize the value of the scientific method—of free inquiry and free discussion, with reference back to fact where possible, as against dogmatic assertion and unreflective assimilation.

Meanwhile, I would suggest that scientific workers in general, and biologists in particular, have a responsibility in seeing that the great generalizations of modern genetics and evolutionary biology, which have so many important implications in so many fields, should be admitted as an indispensable part of general education.

In general, the State must recognize that science has an autonomous aspect. In the first place, it is a quest for knowledge and understanding of Nature as well as a means of controlling it, and some of the most valuable discoveries have been made by men completely unconcerned with possible practical importance, and interested only in discovery for its own sake. In the second place, although scientific progress is obviously conditioned by its social and economic environment, it is not wholly determined by them. Granted that scientific activity is main-

* This applies equally to the political organisation in a one-party State (for example, the Communist Party in the U.S.S.R.), which in some respects is supra-governmental.

tained at or above a certain level, it has a certain momentum of its own, which will automatically lead to the clarification of fields the exploration of which has begun*.

I would suggest that the time is propitious—and also pressing—for men of science all over the world to unite in thinking out these problems, and in particular in defining the scope and degree of the autonomy of science in relation to other higher activities of man, to politics, and to the State. The final outcome should, I think, be a public statement, brief yet comprehensive, a profession of scientific faith, an affirmation of the rights and duties of science and of scientists.

In framing this, the leading role should doubtless be taken by the premier national scientific bodies. Thus, in Britain, we might hope that the Royal Society would take the responsibility. It would presumably wish to consult other bodies which have paid special attention to the subject, such as the Society for Freedom in Science, and would doubtless endeavour to secure as much support as possible from other national societies, such as the British Association and the Association of Scientific Workers, from regional bodies like the Royal Society of Edinburgh, and from the institutes and societies concerned with particular branches of science, especially those dealing with genetics and evolutionary biology.

To put the matter on an international basis, the International Council of Scientific Unions should be called upon, which would then presumably make contact with its various constituent Unions, and with international professional bodies such as the International Federation of Scientific Workers.

A manifesto of this kind, subscribed to by the greatest possible number of scientific organisations and societies, would be a powerful instrument of scientific freedom and an affirmation of the supranational and universal character of scientific activity.

The converse of this should also be undertaken, namely, the formulation of an international charter for science by the intergovernmental agencies concerned—Unesco in the lead, but with the co-operation and agreement of the other specialized agencies concerned (notably the Food and Agriculture Organisation and the World Health Organisation) and of the Economic and Social Council of the United Nations. In this, the Governments concerned would guarantee certain rights to science and to scientists, while presumably at the same time defining certain duties expected from them.

The third and quite specific question remains—how to persuade the U.S.S.R., together with any other countries which may be concerned, to reverse its present policy towards science. Clearly nothing can be done here save by way of precept—through an inter-governmentally agreed charter as suggested above; and of persuasion—through a joint approach of the scientific academies of the rest of the world, and perhaps through the International Scientific Unions. In the particular case of genetics, a joint approach might be made by the specialized societies concerned with genetics and evolution, perhaps with the co-operation of the International Genetics Congress and the International Union of Biological Sciences. Any such approaches should lay strong emphasis on the unity and international character of science, as well as on the advantages of scientific freedom and autonomy *vis-à-vis* political or philosophical orthodoxy.

* Dr. J. R. Baker has directed my attention to the fact that the above suggestions are much along the lines of those put forward by the Society for Freedom in Science during the past nine years.

The actions and the methods of Lysenko and of the Academy of Sciences in supporting him are in my view intellectually and socially deplorable. But if the issues of principle which they have raised are clearly grasped and taken to heart by men of science in general, and the conclusions from them resolutely drawn, the battle of Soviet genetics will not have been fought and lost in vain. For this will mean that over the great majority of the world the scientific movement will for the first time have become fully conscious of itself and of its social functions, of the vital importance, but at the same time the limitations, of scientific method, of the equal importance of a proper degree of scientific autonomy, and of the rights and duties of science in relation to other higher activities of man, to the State, and to human society as a whole.

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CHEMISTRY OF THE HEAVY ELEMENTS

RECENT work on the chemistry of the heavy elements formed one main topic of a symposium, organised by the Chemistry Division of the Atomic Energy Research Establishment, Harwell, on behalf of the Chemical Society, and held at Rhodes House, Oxford, during March 28–30. The discovery of the transuranic elements, neptunium, plutonium, americium and curium, has demonstrated that a new transition series, like that of the rare earth elements, arising from the filling of 5*f*-electron levels, commences at some stage subsequent to actinium (atomic number 89). Any attempt to trace the chemical relationships between these heavy elements, and the elements standing above them in the Periodic Table, shows how fragmentary and inexact is the existing knowledge even of the chemistry of uranium and thorium, although the former was discovered by Klaproth in 1789 and the latter by Berzelius in 1828.

The first session of the symposium was devoted to the elements other than uranium. Dr. R. E. Connick (Radiation Laboratory, University of California) gave a review of the chemistry of the transuranic elements. These display a clear similarity among themselves, as is shown by the existence in solutions of the M^{4+} cations of all elements from uranium to americium, of M^{3+} cations from uranium to curium, and of the oxycations MO_2^+, MO_2^{2+} for the quinquevalent and sexivalent states of uranium, neptunium and plutonium. Curium is invariably trivalent, americium most stable in its trivalent state; and these elements are separable only with difficulty from the rare earth elements—most readily by ion-exchange methods like the separation of the rare earths themselves. The similarity between rare earth and transuranic elements extends to their narrow-banded absorption spectra in solutions. In particular, the resemblance between the absorption spectra of Ga^{3+} and Cm^{3+} suggests that in the latter the electronic group $5f^7$ is present. As was pointed out by Dr. A. G. Maddock (Cambridge) in the subsequent discussion, this would imply that the 5*f*-series virtually commences after actinium ($Z = 89$), even although the crossing-over of the 6*d*- and 5*f*-levels may occur only at some higher atomic number. The variable valency of the first members of the series points to a much weaker screening of 5*f*-levels than of the 4*f*-levels of the rare earth elements.

Dr. M. Haissinsky (Institut du Radium, Paris), in a paper on "The Position of the Cis- and Trans-uranic Elements in the Periodic System", argued that classifications and terminology applied to the heavy elements must conform to the known chemical relationships of the elements. Thorium is the perfect homologue of zirconium and hafnium, uranium that of molybdenum and tungsten, as is confirmed by the new contributions to the chemistry of thorium and uranium presented later to the symposium. To classify these elements, and protoactinium, with actinium in an 'actinide' series, as exemplifying any 'horizontal' relationship, is not logical. There is such a horizontal relationship between uranium, neptunium and plutonium, in their highest valence states, and if this relationship cannot be extended to americium and curium, the reason may, in part, be in our insufficient chemical knowledge of these elements. This point was, indeed, cogent: participants in the discussion were agreed that the study of transuranic elements on a radio-tracer scale in solution, or by ultramicrochemical methods, is at best incomplete, and provides no substitute for the methods of classical inorganic chemistry.

Contributions to the chemistry of thorium and protoactinium followed this discussion. Mr. R. W. M. d'Eye (Harwell) described work showing the existence of compounds of the hitherto unknown bivalent and trivalent states of thorium. Two iodides, ThI_3 and ThI_2, have been characterized analytically and by X-rays, and have been shown to resemble in every respect the corresponding compounds of zirconium, but to have no resemblance to the cerous salts. The evidence suggests that in the Th^{3+} and Th^{2+} cations, the unutilized valence electrons are disposed in 6*d*-orbits; if so, the occupation of 5*f*-orbits cannot begin before protoactinium.

Since it was stated that in the U^{4+} ion two electrons are in 5*f*-orbits, the chemistry of protoactinium assumes considerable interest. Dr. A. G. Maddock and Mr. G. F. Miles discussed the tracer chemistry of protoactinium solutions. The important result obtained was the demonstration that, except in the most acid solutions (for example, 6 *N* hydrochloric acid), protoactinium solutions are hydrolysed to radiocolloids. Coprecipitation is therefore not selective and affords no evidence for isomorphism of protoactinium compounds with those of tantalum, zirconium or other elements. There is no evidence for the formation of soluble protoactinates, analogous to the tantalates, nor—in tracer solutions at least—for the reducibility of quinquevalent protoactinium to any lower valence state. Only in stable complex salts—for example, with citrate, fluoride or cupferron—can protoactinium be brought into true solution. The cupferron complex is soluble in organic solvents, and, in conjunction with coprecipitation of protoactinium on manganese dioxide, should afford a convenient means of isolating protoactinium in macroscopic quantities.

M. Haissinsky and G. Bouissère, working on a microchemical, as distinct from a tracer, scale, reported that protoactinium can be reduced from the quinquevalent to a lower state. Coprecipitation with lanthanum, and separability from zirconium, suggest that Pa^{3+} is formed thereby, the Pa^{3+}/Pa^{5+} potential being not very different from the Cr^{2+}/Cr^{3+} potential.

Subsequent discussion emphasized the need for much more study of the chemistry of protoactinium. Prof. O. Hahn (Göttingen), in discussing the original isolation of protoactinium in his institute in Berlin, pointed out that one well-crystallized compound, K_2PaF_7, at least, and probably the oxide Pa_2O_5 as well, has been definitely characterized. Dr. Maddock recalled an American statement that protoactinium oxide has the fluorite structure. In the view of the reporter, even if this is confirmed, it does not necessarily imply, in the absence of chemical evidence, that the oxide is PaO_2. The chemistry of the heavier transition elements provides numerous instances of oxides with defect structures, so that formulæ based on X-ray data only must be treated with reserve.

The second session of the symposium was devoted to the complex chemistry of uranyl solutions. Mr. J. Sutton (Harwell) discussed the constitution of uranyl perchlorate solutions, as evidenced by cryoscopic measurements, *p*H measurements, conductivities and absorption spectra. In such solutions, the cations $U_2O_5^{2+}$ and $U_2O_5^{4+}$ are successively formed by condensation reactions; further hydrolysis can be interpreted as involving hydroxo-complexes $[U_2O_5.OH]^+$,

$[U_3O_8(OH)_2]$ (in metastable solution) and ultimately the anionic complex $[U_3O_8(OH)_2]^-$. Mr. T. V. Arden (Teddington) and Mr. F. W. Cornish (Exeter) confirmed this general mechanism of hydrolysis, but gave evidence for the formation of still more condensed poly-cations, for example, $U_4O_{11}^{2+}$, in solutions of uranyl nitrate, chloride and sulphate. Dr. R. H. Betts and Miss R. K. Michels (National Research Council, Canada) described measurements on ionization equilibria in uranyl nitrate and uranyl sulphate. Precise absorption spectrophotometry makes it possible to determine the extent of ion-pair formation, and the formulæ of the species concerned can be deduced from the equilibrium data. Measurements of this kind require a knowledge of activity data for concentrated and multi-component solutions. An ingenious method of calculating such data was described by J. F. Duncan, E. Glueckauf, H. A. C. McKay and A. R. Mathieson (Harwell); the uranyl ion activity can be compared directly with that of pure uranyl nitrate solutions by means of liquid–liquid partition equilibria, and the activity of the second electrolyte in a mixture can be compared with that of the uranyl ion by an ion-exchange method. The solubility of uranyl nitrate hexahydrate in ethers and ketone solvents has long been known. Mr. A. R. Mathieson has isolated molecular compounds $UO_2(NO_3)_2.3H_2O.K$ (K = acetone or other ketone molecule) from such solutions, and shown their existence in solution.

In a further session of general chemistry, Dr. F. G. Mann and Mr. J. A. C. Allison (Cambridge) described complex compounds of uranium tetrachloride with tertiary alkyl phosphines, $[UCl_4(R_3P)_2]$, analogous to those formed by $SnCl_4$. A paper on the lower oxides of uranium, by Mr. K. B. Alberman (Harwell), revealed a complex system comparable with that of the oxides of molybdenum and tungsten. Oxidation of uranium oxide, UO_2, proceeds at quite low temperatures, apparently by a diffusion-controlled process. A non-stoichiometric oxide phase is thereby formed, with cubic symmetry up to the composition $UO_{2\cdot3}$, becoming tetragonal with increased oxygen content up to the level $UO_{2\cdot25}$, at which oxidation ceases. These oxides are unstable above 250°, and split up into two-phase mixtures of UO_2 proper, a well-defined intermediate phase with composition close to $UO_{2\cdot26}$, and the previously known U_3O_8 or U_3O_7 phase. Dr. H. A. Wilhelm (Iowa State College) reviewed the high-temperature phase equilibria in the uranium–carbon and thorium–carbon systems. UC, with rock-salt structure, forms solid solutions with UO and UN; UC_2 appears able to incorporate carbon in excess of the ideal formula. It was pointed out in discussion that this suggests an analogy with sulphide chemistry, in the ability of pyrite-type sulphides to incorporate an excess of sulphur. The miscibility of UC and UC_2 at high temperatures, or the possible existence of an intermediate phase U_2C_3, is shown by the beautiful Widmanstätten unmixing structures in materials of that composition.

The only contribution on polonium chemistry came from Dr. P. Bonet-Maury and M. Leport (Paris). The formation of hydrogen peroxide from water by the action of polonium α-rays is inhibited if there is direct contact of the solution with the polonium source, and polonium has a strong specific catalytic effect on the decomposition of hydrogen peroxide. Dr. Haissinsky pointed out that true peroxides are formed only by elements with electronegativities less than that of hydrogen. The action of polonium might be interpreted if, as an exception to the general rule, it formed a per-compound from tetravalent polonium rather than from the highest valence state. Dr. R. E. Connick (Berkeley) cited the reduction of plutonium (VI) compounds, with destruction of hydrogen peroxide, as an analogy: Pu(VI) $\xrightarrow{H_2O_2}$ Pu(V) → Pu (VI) + Pu (IV): Pu (IV) is then either converted to peroxide or reduced to Pu (III). Dr. Maddock, however, inquired whether the chemical effects of the recoiling polonium atoms were not of some possible importance.

Analytical aspects of the subject were dealt with in two papers. Dr. R. B. Jacobi (Harwell) described improved techniques for the determination of radon and radium in natural waters. Mr. T. V. Arden, Dr. F. H. Burstall and Dr. R. P. Linstead (Chemical Research Laboratory, Teddington) gave an account of a new and highly selective method for the detection and determination of small amounts of uranium, based on their use of paper chromatography. When an organic solvent is allowed to diffuse over a strip of filter paper, or a column of pulp, impregnated with a dilute nitric acid solution of the sample, the high solubility of uranium in the solvent makes this element move with the advancing front of solvent, so that the appropriate section of paper can be 'developed' to reveal uranium, or isolated, ashed and subjected to quantitative micro-analysis. The movement of most other elements is relatively small, and the method is useful in the analysis of low-grade minerals and similar materials. J. S. ANDERSON

OBITUARIES

Prof. Felix d'Herelle

THE death in Paris on February 22 has been announced of Prof. Felix d'Herelle, best known for his monumental work on bacteriophage.

Felix d'Herelle was born at Montreal, Canada, in 1873, of French-Canadian parentage, and though he retained throughout life his British nationality, of which he was always proud, he gave to his acquaintances the impression of being rather more French than British in temperament and outlook. Educated at the Lycée Louis le Grand, Paris, he took his baccalauréat in 1888, after which he returned to his native Canada to take up the study of medicine and to graduate M.D. at Montreal. During 1901–7 he was professor of bacteriology at Guatemala, to become bacteriologist to the Mexican Government during 1908–9. In 1909 he joined the staff of the Pasteur Institute, Paris, as assistant, to be promoted later to *chef de laboratoire* (1914–21). During these years his services were much in demand to go on special missions, at the request of various foreign Governments. Thus he visited the Argentine (1911–13), Turkey (1914), Tunis (1915) and Indo-China (1921). Then he went to the University of Leyden as visiting professor during 1922–23. In 1924 he was appointed to the International Quarantine Council, Egypt, as director of laboratory service, with a laboratory at Alexandria. Here he remained until 1926, when he went to India on a special mission at the request of the British Government. In 1928 he was appointed professor of protobiology at Yale University, from which post he retired in 1933 to found the Laboratoire du Bacteriophage, Paris. Between 1934 and 1936 he was entrusted by the Government of the

U.S.S.R. with the setting up of bacteriophage research institutes at Tiflis, Kiev and Kharkov.

In 1917 in a note to the French Academy, d'Herelle published his epoch-making observations "Sur un microbe invisible, antagoniste des bacilles dysenteriques", wherein he gave a description of the phenomenon of bacteriophage which bears his name. The phenomenon of bacteriophage had no doubt been discovered earlier by Twort (1915), but the graphic description given by d'Herelle, on his independent discovery of it, startled the scientific world to the extent that the discovery was immediately appreciated as one of the greatest of the century. In this connexion it is worthy of remark, in passing, that through two of her sons, d'Herelle and Banting, Canada stands associated with the two most outstanding and far-reaching medical discoveries of the first quarter of the present century: bacteriophage and insulin.

A recipient of many honours, there were two of which he was specially proud: the Leeuwenhoek Medal of the Amsterdam Academy of Science (1925), and the M.D. of Leyden (1923). The former he valued because the only previous French man of science to receive the distinction was Louis Pasteur (1895); and the latter because among the very few British recipients of a doctorate of the University of Leyden was Mr. Winston Churchill.

Of d'Herelle's better-known English writings, mention may be made of "Immunity in Natural Infectious Disease" (1924), which was followed by "The Bacteriophage and its Behaviour" (1926), "The Bacteriophage and its Clinical Applications" (1930), "Studies on Asiatic Cholera", the latter in conjunction with Malone (1930); while his main writings in French were: "Le Bacteriophage, son rôle dans l'Immunité" (1921), "Les Defences de l'Organisme" (1923), "Le phénomène de la guerison dans les maladies infectieuses" (1938), "Etude d'une maladie: le cholera, maladie aux paradoxes" (1948).

For reasons difficult to understand, the therapeutic aspects of his work never enjoyed the support and enthusiasm in Britain that they achieved elsewhere, which must have been a deep disappointment to him, though he avoided speaking of it in that way.

A clear thinker, with an impeccable laboratory technique, and endowed in high degree with the logical outlook of his race, d'Herelle appeared at times somewhat intolerant to those who differed from him; yet, to his friends and associates, he was one of the kindest and most loyal of men. To have known him at all well was indeed a 'liberal education' and a very great privilege.

In 1893 d'Herelle married Marie Caire, who predeceased him; and they are survived by two daughters and three grandchildren. A. COMPTON

Dr. R. P. Penrose

REGINALD PERCY PENROSE, whose premature death occurred at Wassenaar, near Leyden, on April 28, was educated at Altrincham Grammar School, where his distinguished scholastic work was crowned by a Major Open Scholarship to the Queen's College, Oxford. He went into residence in 1939, and took the shortened Final Examination in Physics two years later. He then joined the Admiralty research group at the Clarendon Laboratory, Oxford, where he assisted in the design of test equipment for centimetre wave magnetrons.

Towards the end of the War, Penrose developed a method of measuring the dielectric properties of solids at centimetre wave-lengths, using an H_0-cavity resonator. He pushed this technique a stage further in the analysis of the inversion spectrum of ammonia, the first step in opening up the field of microwave spectroscopy. The award of a Senior Studentship of the 1851 Exhibition in 1946, the year in which he received the degrees of M.A. and D.Phil., enabled him to continue his researches in Oxford on collision broadening, while a year later he turned to break fresh ground in low-temperature work on paramagnetic resonance. He went to Leyden last autumn on an exchange scheme between the Clarendon Laboratory and the Kamerlingh Onnes Laboratory with the intention of continuing his work there for six months, before taking up a lectureship at the University of Leeds. Just before Christmas he discovered the hyperfine structure of the paramagnetic resonance line in a diluted crystal of copper potassium sulphate, that is, the effect due to the interaction of the nuclear spins with the electron spins (see p. 992 of this issue of *Nature*). This was the first time such an effect had been observed in an atomic transition in the solid state. The publication of his work, however, was delayed by his illness. He had to undergo an operation, and although he appeared to be making a complete and successful recovery, he collapsed suddenly in the hospital at Wassenaar a few days later.

Dr. Penrose's chief relaxations were photography and cycling, and there were few byways near Oxford unknown to him. His life centred in his research, however, and his enthusiasm in following up a new idea led him to make much of the apparatus himself, for he was a skilful machinist. A cheerful companion, he was liked by all; I was particularly fortunate in enjoying four years of close partnership with him in the Clarendon Laboratory. Our intimate relationship did not cease when he left Oxford, and the exchange of results and ideas with him will be greatly missed. His death at the age of twenty-eight deprives microwave physics of one of its exponents, and his scientific papers (some of them yet to appear) form the memorial to his pioneer work in this field.

B. BLEANEY

Dr. A. D. Imms, F.R.S.

DR. M. AFZAL HUSAIN, president of the Pakistan Biological Society and of the Pakistan Association for the Advancement of Science, writes:

The news of Dr. A. D. Imms's death has been received with deep sorrow in entomological circles in Pakistan. Besides making outstanding contributions to Indian entomology in his capacity as a former forest entomologist in India, Prof. Imms trained under his inspiring guidance many workers prominent to-day in the field of entomology. The magnitude of his contribution to the development of entomology in Pakistan and the other countries of the Commonwealth in his capacity as a teacher at the University of Cambridge will probably outweigh in value even his direct contributions to the science of entomology, outstanding as these latter are. The numerous schools of investigation and research he thus helped to bring into being in the various parts of the Commonwealth will, I am sure, long remain as monuments to an extremely creative and inspiring life.

NEWS and VIEWS

Medical Research Council and Sir Edward Mellanby, G.B.E., K.C.B., F.R.S.

SIR EDWARD MELLANBY will shortly be retiring from the secretaryship of the Medical Research Council, a post which he has held for some sixteen years. He has, however, served and inspired the Council for close on thirty-five years. He was invited by the parent body of the Council, the short-lived Medical Research Committee, to undertake an inquiry into the cause of rickets soon after the beginning of the First World War, and his remarkable success in solving this problem soon gained him a world-wide reputation. He became well known during the inter-war years as a leading exponent of the new science of nutrition. This is not the place to tell of Sir Edward's own important contributions to medical and physiological science—original work which he has pursued vigorously and fruitfully during all his years of administrative work at the Medical Research Council. Rather should it here be remembered that his fearless devotion to the pursuit of truth has stimulated and directed medical research in Great Britain along many and varied tracks with a gratifying degree of success. Those with genuine medical or physiological problems to tackle would not apply to Sir Edward in vain for a hearing. He might conduct searching and sometimes even chastening interviews with aspirants to medical research; but if he were convinced of the soundness of an idea and the integrity of purpose of its sponsors, he would be prepared to recommend the support of the man and the work with all the resources of his Council. Sir Edward will be sorely missed in his administrative capacity. There is, however, every reason for believing that he will still be able to give much of his time to his own medical research.

Prof. H. P. Himsworth

PROF. H. P. HIMSWORTH, who succeeds Sir Edward Mellanby as secretary of the Medical Research Council, has been since 1939 professor of medicine in the University of London and director of the medical unit at University College Hospital. It was from this Hospital that he qualified in 1928, obtaining the University Medal in both the M.B. and M.D. examinations. After holding the usual resident appointments, he started research into the mechanism of diabetes mellitus. The value of this work was recognized by the award of the Julius Mickle Fellowship by the University of London in 1935, and the results of his investigations were summarized in the Goulstonian Lectures, which he gave to the Royal College of Physicians in 1939, and in the Oliver Sharpey Lectures delivered last spring. During the War, his interest turned to the study of liver disease, using both the clinical and experimental approach. He has done outstanding work on the relation of nutrition to the causation of liver disease, differentiating clearly, in man and in the experimental animal, between the various types of degeneration and chronic fibrosis in the liver. His Lowell Lectures, which he gave in Boston, Massachusetts, in 1947, formed the basis of his monograph on diseases of the liver. He has also worked extensively on the treatment of thyroid disease with the new anti-thyroid compounds. In 1948, Prof. Himsworth was elected a member of the Medical Research Council, a body which he had already served in many capacities, for he was chairman of the Protein Requirements Committee and secretary of the special advisory committee on the medical aspects of food rationing. During the tenure of his present post at University College Hospital, he has shown himself keenly interested in medical education and in the advancement of clinical medicine. His wide clinical experience, his record of brilliant research work and his well-recognized administrative ability have fitted him excellently for the responsible post to which he has been appointed.

The Earhart Plant Research Laboratory

ON June 7 at the California Institute of Technology a new laboratory was opened for experimental botany. It is under the direction of Dr. F. W. Went. Dr. R. A. Millikan, a past president of the Institute, has called it a "phytotron" in the expectation that it may do for botany what the cyclotron has done for physics. The laboratory consists of six air-conditioned greenhouses and thirteen air-conditioned growth chambers, in which there can be produced various conditions of light intensity and quality, temperature and humidity, besides artificial rain, wind and fog. Nutrient solution is laid on to each chamber from a 1,000-gallon tank. There are arrangements for the routine photographing of plants in each growth chamber. Elaborate precautions are taken to exclude plant diseases and pests from the laboratory. The laboratory cost half a million dollars to build and will cost about 40,000 dollars a year to run. Its construction involved the solution of several difficult engineering problems. Of particular interest to plant physiologists is the claim that artificial illumination of 2,000 foot-candles can be secured by the use of fluorescent tubes operated at about four times the normal current density, supplemented by a few 60-watt incandescent lamps. It is stated that the life of fluorescent tubes under these conditions is still as much as 1,920 hours. Plant physiologists will await with interest the results of research from this novel laboratory.

American Technical Help in Colonial Development

THE Secretary of State for the Colonies, Mr. A. Creech Jones, announces that Dr. W. V. Lambert, dean of the College of Agriculture, University of Nebraska, Dr. W. A. McCall, assistant chief of the Bureau of Plant Industry, Soils and Agricultural Engineering, of the U.S. Department of Agriculture, and Dr. Arthur H. Cline, professor of soil science, Department of Agronomy, Cornell University, are to conduct a three-months survey of farming conditions in East, Central and West Africa, beginning in July. The Survey, which is part of the technical assistance programme of the Economic Co-operation Administration, is being undertaken at the request of the United Kingdom Government. It is designed to help the agricultural and veterinary departments of the East, Central and West African territories in the general field of production and in developing veterinary research, plant pathology and soil science, by finding out how best American technical agricultural assistance can be applied. This is the second project to be approved under the Administration's technical assistance scheme. The first, announced on May 16, provided for a six-weeks visit to West Africa and East and Central Africa respectively of an American scientific worker and an insecticide expert.

"Colonial Month"

"COLONIAL MONTH", which was inaugurated by the King at Church House, Westminster, on June 21, represents a concerted effort of various societies, public institutions, firms, etc., to demonstrate to the public in Britain the products of the Colonies of Britain and the ways of life of their peoples. The Exhibition Hall of the Central Office of Information at Oxford Street, Marble Arch, W.1, provides the centrepiece of Colonial Month with its "Focus on Colonial Progress" Exhibition, which will be open until July 30. In addition, there will be special displays of Colonial products in London shop windows; many large London stores are arranging displays of books and colonial products, which include veneers, cocoa and Sea Island cotton.

In connexion with the "Colonial Month", an exhibition entitled "Colonial Geology and Mineral Resources" is being held at the Exhibition Pavilion, Imperial Institute, South Kensington, London, S.W.7, until July 20. This exhibition, which is organised by the directorate of Colonial Geological Surveys, illustrates the work of the directorate and the Survey, with particular reference to the development of mineral resources. The exhibition, which is free, is open during 10 a.m.–4.30 p.m. on Mondays to Fridays, and 10 a.m.–1 p.m. on Saturdays. Another exhibition, on "Traditional Art of the British Colonies", is open at the Royal Anthropological Institute (see *Nature*, June 18 (p. 948)).

Great Ross Ice Barrier

THE problems of origin, maintenance and wastage of the Great Ross Barrier of the Ross Sea are the subject of a detailed study by Prof. F. Debenham in the *Geographical Journal* of April 1949. The studies have been much advanced by the observations and measurements made by recent American expeditions. Roughly speaking, the width of the ice-area reaches 400 miles, and its north and south length, on its widest eastern side, 700 miles. It is a vast raft of ice, thin compared with its area, loosely held by its bounding lands and more securely moored by islands and shoals. It is slowly moving towards the Ross Sea, but at a rate that varies in different parts of its front. At the free edge the rate may reach 15 ft. per day. This is due to accessions of snow and also by the creeping, or slumping, of the ice under its own weight. But the most striking part of the paper deals with the likelihood of accretions in thickness due to growth by freezing of the waters below the Barrier. Several phenomena support the belief that this freezing does occur; for example, the marine plants and shells on the surface of the ice, patches of mirabilite (sodium sulphate) originating in minor bottom depressions when freezing concentrates the water and precipitates the minerals, and lastly curious remains of fishes found on the surface but clearly emanating from seal attacks in the water. These sources of evidence are fully discussed in this paper, as well as the rise of the Barrier ice, as it grows, and its outward flow.

'Escape' from the Take-all Disease

IT has been shown by S. D. Garrett (*Ann. App. Biol.*, 35, No. 1, 14–17; March 1948) that the severity of root attack by the take-all fungus, *Ophiobolus graminis*, is reduced by applications of nitrogen, and by combined dressings of phosphate and potash. This state of affairs was observed in field experiments, but not in pot cultures where the fungus was inoculated on the crowns of barley plants. The discrepancy is interpreted by assuming that in the field the added fertilizers stimulate the plant to make new roots faster than the fungal parasite can attack them, and so the host 'escapes' from the fungus.

American Birds

THE Chicago Field Museum of Natural History has recently published the third part of its catalogue of birds of the Americas and adjacent islands. The catalogue contains accounts of all the species and sub-species known to occur in North America, Mexico, Central America, South America, the West Indies and Islands of the Caribbean Sea, the Galapagos Archipelago, and other islands which are included because of their faunal affinities. In the present number, descriptions of the families Jacanidae – Rostratulidæ – Hæmatopodidæ, Charadriidæ – Scolopacidæ – Recurvirostridæ, Phalaropodidæ – Burhinidæ – Thinocoridæ – Chionididæ, Stercorariidæ – Laridæ – Rynchopidæ – Alcidæ are given. The final volume, containing descriptions of the Falconiformes, is now in course of publication.

Research on Whale Products

THE Department of Scientific and Industrial Research is considering establishing on a more permanent basis the section of the work of the Food Investigation Organisation at the Low Temperature Research Station, Cambridge, dealing with food products from the whale other than oil. A laboratory on board the F.F. *Balaena* has been operated by the Low Temperature Research Station during the last three whaling seasons on a year-to-year basis. Longer range research, mainly of a biochemical character, is now planned. Further information about the kind of work contemplated can be obtained by research workers and others interested in this new field from the Superintendent of the Low Temperature Research Station.

Conference of the South-Western Naturalists' Union

THE twenty-second annual conference of the South-Western Naturalists' Union was held at the Pump Room, Bath, during June 3–6, by invitation of the Bath Natural History Society. The conference was opened with a reception given by the Mayor of Bath, Councillor L. N. Punter, at which Mr. W. G. Rawlings gave a talk on "The Nightingale". Much of the conference was devoted to excursions, including Cherhill Down, Avebury and Lacock, and botanists were gratified at the number of different species of orchids and other chalk-loving plants that were found. At the general meeting on June 6, the president of the Union, Sir Lewis Fermor, gave his presidential address, the subject being "Water". In this address attention was given to the natural-history aspects of water, under such headings as beauties, benefits, damage due to water, conservation, underground water, solution with production of caves, and hot springs. Special stress was laid on the necessity for controlling the destructive action of water that causes soil erosion and flooding, and on the necessity for engineering measures of conservation by which countries that are now deficit countries for food production may become self-supporting. Mr. Peter Scott was elected president for 1950, and Marlborough was selected as the venue for the annual conference for Whitsuntide, 1950, at the invitation of the Marlborough College Natural History Society.

University of Birmingham: Appointments

THE following announcements have been recently made in the University of Birmingham. The title of reader in astrophysics has been conferred on Dr. M. C. Johnson, lecturer in physics. *Appointments:* Prof. F. W. Shotton, of Sheffield, to the chair of geology in succession to Prof. L. J. Wills; W. I. B. Smith, to be Nuffield Foundation Fellow in the Department of Physics; Dr. D. H. Whiffen, to be lecturer in physical chemistry; W. R. Cheetham, to be lecturer in mining; J. G. Jackson, to be lecturer in pathology; Miss V. Norris, research fellow in the Department of Medical Statistics, to be senior statistical officer. *Resignations:* Dr. C. H. Westcott, Nuffield Research Fellow, on appointment as associate professor in the Department of Physics, McGill University; and Dr. R. Padley, senior statistical officer in the Department of Medical Statistics, on appointment as deputy statistical officer to the City of Birmingham Corporation. G. de Q. Robin, lecturer in physics, has been given leave of absence to join the Anglo-Norwegian expedition to Queen Maud Land. G. T. Warwick is leading an expedition to Lyngen Fjord, Northern Norway, this summer; the party will consist of seven final-year geography students and a Norwegian student from the Civil Engineering Department. The objects of the expedition will be to make a study of the geomorphology, human geography and microclimatology of the region. H. Thorpe is visiting Denmark and Sweden during the summer vacation to continue research into forms of rural settlement, with particular reference to Green Villages.

University of Leeds: Appointments

THE following appointments have recently been made in the University of Leeds: Mr. W. R. Atkin, to succeed Prof. D. McCandlish, who retires at the end of the present session, in the chair of leather industries; A. S. Carson, to be lecturer in inorganic and physical chemistry; Dr. N. J. Petch, to be reader in metallurgy; W. K. J. Walls, to be lecturer in anatomy. An offer by Messrs. Bolton Leathers, Ltd., to establish a postgraduate fellowship in the Leather Industries Department, to the value of £450–£600 a year for a period of three years in the first instance, has been accepted; the award will be known as "The Boltonia Fellowship".

Conference on Amino-Acid and Protein Hydrolysates

THE Food Group of the Society of Chemical Industry is arranging a conference on "Amino Acids and Protein Hydrolysates", which will be held in the William Beveridge Hall, University of London, Senate House, London, W.C.1, during September 28–30. Papers will be presented upon the chemistry, properties, preparation, analysis, industrial applications and nutritive aspects of amino-acids and protein hydrolysates, and it is hoped that visits may be arranged to laboratories engaged upon work in this field. A detailed programme and application forms for registration will be issued in due course; the honorary secretary of the conference is Dr. A. J. Amos, 88 Madeley Road, London, W.5.

Second International Biometric Conference

THE Second International Biometric Conference will be held in the University of Geneva during August 30–September 2, under the auspices of the Biometric Society. Aspects of biometry which will be considered in the scientific sessions include recent applications in genetics, teaching and education, experimental design, the present status of the science, industrial applications and biological assay. The Conference will close with a session of contributed papers. Further information and reservations are obtainable through the secretary of the Conference Committee, Prof. Arthur Linder, 24 Avenue de Champel, Geneva, Switzerland.

The Night Sky in July

FULL moon occurs on July 10d. 07h. 41m., U.T., and new moon on July 25d. 19h. 33m. The following conjunctions with the moon take place: July 11d. 02h., Jupiter 5° N.; July 23d. 11h., Mars 4° S.; July 27d. 18h., Venus 2° S.; July 28d. 01h., Saturn 2° S. In addition to these conjunctions with the moon, Venus is in conjunction with Saturn on July 31d. 06h., Venus 0·2° S. Mercury rises an hour before the sun during the first half of the month and later on becomes an evening star, but sets too soon after sunset to be visible. Venus sets more than an hour after the sun on July 1 and 31, and can be seen as an evening star throughout the month; about nine-tenths of the illuminated disk is visible during July. Mars is a morning star, rising at 2h. 10m., 1h. 50m. and 1h. 33m. at the beginning, middle and end of the month, respectively. The planet is fairly close to ζ Tauri towards the middle of the month. Jupiter, in the constellation of Sagittarius, rises in the early night hours and can be observed low down in the heavens; but as the planet does not attain an altitude greater than 18° in the latitude of Greenwich, it is not favourably placed for observation in higher northern latitudes. It is in opposition to the sun on July 20. Saturn, in the constellation of Leo, is visible during the early portion of the night, setting at 22h. 45m. and 20h. 52m. at the beginning and end of the month, respectively. Occultations of stars brighter than magnitude 6 are as follows: July 6d. 22h. 44·7m., 40B. Scor. (*D*); July 21d. 01h. 17·4m., 133B. Taur. (*R*). *D* and *R* refer to disappearance and reappearance, respectively, and the latitude of Greenwich is assumed. The earth enters aphelion on July 2.

Announcements

DR. S. C. HARLAND, F.R.S., since 1939 director of the Institute of Cotton Genetics, National Agricultural Society, Peru, has been appointed reader in genetics in the University of Manchester.

DR. REGINALD CHILD has relinquished the post of director of the Coconut Research Scheme, Ceylon, which he has held since 1932. He will be succeeded by Mr. F. C. Cooke, who was chemist (coconut products) with the Department of Agriculture, Malaya, during 1929–38, and canning officer of the same Department during 1946–49.

FOLLOWING upon Prof. Niels Bohr's Gifford Lectures, to be delivered in Edinburgh during October 21–November 11, a conference on elementary particles will be held in the Department of Natural Philosophy, University of Edinburgh, during November 14–16. Further information will be available about the end of August and can be obtained from Mr. A. Nisbet, Department of Mathematical Physics, University of Edinburgh, Drummond Street, Edinburgh 8.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

The Light Emitted by Europium Compounds

SOME time ago, my co-workers and I [1] found that the well-known blue fluorescence band of some fluorites is due to traces of bivalent europium. In later papers[2], I showed that europium can easily be reduced to the bivalent form by heating in various basic materials.

F. Weger, in this laboratory, has studied the fluorescence spectrum of Eu^{++} in various substances. There is always a broad band in the blue, but the position of the maximum varies widely with the nature of the basic material and with the way the sample has been heated; heating on a platinum wire in the flame shifts the maximum towards longer wave-lengths as compared with a sample heated in a glass tube.

This investigation was well advanced when I saw the important paper by S. Freed and S. Katcoff[3] on the absorption and fluorescence spectra of bivalent europium. These authors, too, note the strong dependence of the maximum on the nature of the crystalline surroundings, and give for the bands of Eu^{++} the interpretation proposed by me[4] in 1936, namely, transitions between $4f$ and $5d$, etc.

With a mixture of calcium chloride and borax containing traces of europium, we get by suitable heating a bright yellow fluorescence with a maximum somewhere in the near infra-red for which we have as yet no interpretation.

A borax bead with traces of europium glows, when heated on a platinum wire loop in the blow-pipe flame, with a blue light[5] nearly as intense as the blue component of the light emitted by the glowing platinum wire; the light resembles in colour the fluorescence excited by ultra-violet. This might be chemiluminescence (the candoluminescence of Nichols and his co-workers); but it might also be an extreme case of selective temperature radiation as, according to Freed and Katcoff, the absorption of bivalent europium is very strong in the near ultra-violet[6].

A more detailed account appears in the *Wien. Anzeiger* of December 2, 1948, and in the *Acta Physica Austriaca*.

K. FRZIBRAM

II. Physics Institute,
University of Vienna.
Jan. 5.

[1] Haberlandt, H., Karlik, B., and Przibram, K., *Wien. Ber.*, IIa, **143**, 151 (1934).
[2] Przibram, K., (*a*) *Wien. Ber.*, IIa, **144**, 141 (1935); (*b*) *ibid.*, **147**, 261 (1938).
[3] Freed, S., and Katcoff, S., *Physics*, **14**, 17 (1948).
[4] Przibram, K., *Z. Phys.*, **102**, 331 (1936).
[5] Przibram, K., *Wien. Anz.*, 98 (1946). In this paper ref. 2 ought to read: Nichols, E. L., and Boardman, L. J., *J. Opt. Soc. Amer.*, **20**, 115 (1930).
[6] Compare also Przibram, K., ref. 2(*b*).

Formation of Microscopic Crystals on Crystal Surfaces Exposed to α-Rays

DURING some experiments on the coloration of rock salt by the α-rays of polonium, we noticed that the surface exposed to the rays in air became dull. Examined under the microscope, it was found to be covered by minute birefringent crystals which could be identified as sodium nitrate. Similar crystals are also formed on exposing rock salt to the action of nitric acid fumes or to nitrogen dioxide prepared by heating lead nitrate. The crystals obtained when exposing rock salt to α-rays are evidently due to the formation of nitrogen dioxide by the rays in air. The action of the α-rays on the rock salt surface seems, however, also to intervene; various samples of rock salt that show differences in the rate of colouring by the rays also show differences in the number of microscopic crystals formed, while no difference is found when nitrogen dioxide alone acts on the same samples. Sometimes the microscopic crystals show a marked orientation on the rock salt surface.

On potassium chloride, crystals of potassium nitrate are formed. On potassium bromide, however, there appear isotropic triangular crystals which cannot be potassium nitrate (birefringent), their melting point also being much higher. Such isotropic triangular crystals are also occasionally found on sodium chloride and potassium chloride; so far we have not been able to identify them.

On fluorite and on calcite we have obtained anisotropic crystals, which cannot be the known form of calcium nitrate, as this substance is isotropic.

A more detailed account of this work appears in the *Wien. Ber.* and the *Acta Physica Austriaca*. We are continuing these investigations.

L. WIENINGER
N. ADLER

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Jan. 5.

Decoloration and the Ultramicroscopic Behaviour of Natural Blue and Violet Rock Salt

THE absorption spectrum of blue rock salt from Stassfurt was determined photo-electrically in the natural state and after heating for a certain time at various temperatures. The spectrum shows the well-known high maximum at 653 mμ and the smaller one at 535 mμ. On increasing the time of heating, the first maximum begins to shift towards shorter wave-lengths; its intensity increases at first and then decreases. The second maximum does not alter its position; it decreases immediately and disappears sooner than the first. The violet salt from the Grimberg mine in the Werra valley behaves quite differently. It shows only one maximum, at 583 mμ, which is not displaced after heating, and begins to decrease immediately.

The ultramicroscopic behaviour of the two sorts of salt is also quite different. The blue salt from Stassfurt shows the well-known bright red Tyndall cone, the violet one at the most a colourless cone not brighter than that sometimes observed in colourless rock salt. The following experiment makes the difference still more impressive: by heating, one can make the blue salt turn violet, with an absorption spectrum identical with that of the natural violet salt. But the former sample shows now a bright yellow Tyndall cone; the latter, as already mentioned, only a weak colourless one.

This seems to support the assertion made by K. Przibram[1] a long time ago, that there is an intermediate stage between the primary amicroscopic yellow colour (*F*-centres) and the colloidal blue, that is, an amicroscopic violet or blue colour, perhaps due

to colour centres at strongly disturbed regions of the lattice; these might possibly be the *R*-centres of F. Seitz[2].

A more detailed account will be given in the *Wien. Ber.* and the *Acta Physica Austriaca*.

N. Adler

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Jan. 5.

[1] Przibram, K., *Wien. Ber.*, IIa, **128**, 483 (1929).
[2] Seitz, F., *Rev. Mod. Phys.*, **18**, 384 (1946).

Scintillation Counters Using Organic Compounds

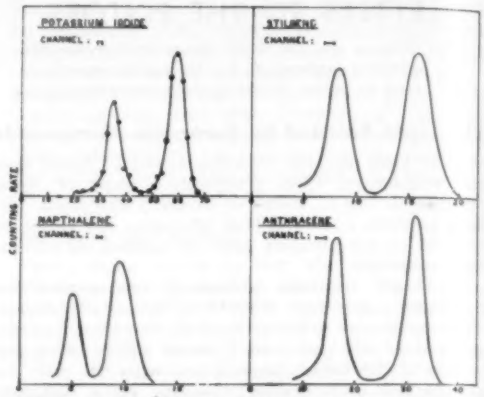
Several papers have recently been published[1-4] on the efficiency of naphthalene, anthracene and other organic phosphors used in conjunction with photomultipliers as detectors of ionizing radiations. In view of the conflicting nature of some of these results, we have carried out experiments with these substances, particular attention being paid to their purity. The phosphors investigated were naphthalene, anthracene and stilbene, and the measurements were made using an EMI Type 4140 photomultiplier with a single-channel pulse analyser to record pulse-height distributions.

The effects of various purification methods were at first investigated by measuring the pulse distributions from cobalt-60 γ-rays, using the phosphors in powder form. This method was found to be quite sensitive to the degree of purification employed and enabled us to make fairly rapid tests of the purity. However, the continuous nature of the pulse distribution was such that it was difficult to make any definite comparisons of the ultimate efficiencies of the different phosphors for light production.

Distributions obtained with α-particles offered a more promising means of comparison[3], and measurements were therefore made with α-particles from polonium (5·3 MeV.) and from thorium C+C′ (6·04 and 8·78 MeV.). For these measurements thin crystals about 2 mm. square and 15 mgm./cm.² thick, selected for uniformity and transparency, were used in order that the β- and γ-ray background should be negligible. The accompanying graphs show pulse-height distributions obtained with naphthalene, anthracene and stilbene, exposed to α-particles from thorium-active deposit. A similar curve obtained with potassium iodide + thallium is included for comparison; we are indebted to Mr. J. Sharpe of this Establishment for providing this crystal. The substances used for these curves were in the purest form we have thus far obtained.

The purification methods attempted were sublimation, repeated crystallization and co-distillation, the last being by far the best, both as to ease and purity obtained. For example, in the case of anthracene, one co-distillation of the commercial product with ethylene glycol was found to give better results and a much higher yield than four sublimations. Co-distillation with ethylene glycol was used for anthracene and stilbene, and with water for naphthalene. It was found slightly advantageous to perform two such distillations, after which crystals were grown from solution: anthracene from ethyl acetate, stilbene from ether, and naphthalene from alcohol.

The curves shown were taken under the same conditions, and the ratios of pulse-heights for the thorium C′ α-particle group are approximately



Pulse-height distributions in single crystals of α-particles from thorium C + C′

8 : 4 : 2 : 1 for potassium iodide, anthracene, stilbene and naphthalene, respectively. It is felt that these ratios, obtained from monochromatic α-particle groups, are more reliable than those obtained with β- or γ-rays. We found, however, that anthracene gave larger pulses than stilbene with both γ-rays and α-particles.

Further preliminary measurements indicate that whereas potassium iodide + thallium gives a linear relation between pulse-height and α-particle energy, the organic phosphors do not. However, it is hoped that the non-linearity will be much less pronounced with protons having a lower ionization density, and that organic phosphors may therefore be used to detect and measure the energy of fast neutrons producing recoil protons in the phosphors.

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B. H. Flowers
J. S. Eppstein

Atomic Energy Research Establishment,
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March 28.

[1] Bell, *Phys. Rev.*, **73**, 1405 (1948).
[2] Irvine and Sangster, *Bull. Amer. Phys. Soc.*, **24**, No. 2, 9 (1949).
[3] Gittings, Taschek, Ronzio, Jones and Masilun, *Phys. Rev.*, **75**, 205 (1949).
[4] Kallmann, *Phys. Rev.*, **75**, 623 (1949).
[5] Broser and Kallman *Nature*, **163**, 20 (1949).

Slow Neutron Monitoring with Boron- and Lithium-loaded Nuclear Emulsions

The rapid development of atomic energy in progress all over the world has posed a major problem in radiation hazard control. Knowledge of the safe dose-levels is, in some cases, not as quantitative as could be desired. Doses of both slow and fast neutrons are in this category[1], and it is therefore important to improve this situation by obtaining a history of the doses received by those individuals exposed to neutrons in the course of their work.

For such purposes a simple detection device, similar to the film badge long used for X- and γ-radiation, is desirable. Development of the technique of particle detection by photographic plates[2] has provided a method of measuring a slow neutron flux by using emulsions loaded with a known mass of a suitable element of known cross-section. Thus J. S. Cheka[3], at the Clinton Laboratories, employed the N^{14} *np* C^{14}

reaction occurring in the nitrogen content of the emulsion to indicate slow neutron exposures of personnel.

The production of lithium- and boron-loaded emulsions by Messrs. Ilford has led to the suggestion[4] that the slow neutron reactions Li^6 $n\alpha$ H^3 and B^{10} $n\alpha$ Li^7 would be preferable. Not only is the yield of tracks higher than in the N^{14} np reaction, but also the α-particle tracks are denser and easier to observe than the short-range protons. For current loadings available in Ilford *C2* plates, the position is summarized in Table 1, from which the advantages of boron and lithium loadings are apparent.

Table 1. Relative yields of nitrogen, lithium and boron, slow neutron reactions (normal isotopic mixture)

Reaction	Loading of element (gm./c.c.)	Relative density of events	Length of track microns	Nature of track
N^{14} np C^{14}	0·067	1	6	proton
Li^6 $n\alpha$ H^3	0·016	18·4	42	α-particle and triton
B^{10} $n\alpha$ Li^7	0·023	183	8	α-particle

Experiments in this laboratory, with 50 μ Ilford *C2* emulsions loaded with boron and lithium on plates 1 in. × 1½ in., have shown that a single observer, working full-time, can determine the fortnightly thermal neutron doses received by a hundred individuals.

In these experiments a plate of each type is given to the subject and is worn for two weeks; it is then collected, processed and counted under a binocular microscope. In the large-scale application of the method, a simple projection microscope would be used.

Boron-plates are normally examined employing an oil immersion 2-mm. objective viewing a field of area 10^{-4} cm.^{2}. Because of the longer tracks in the lithium-plates a 4-mm. objective can be used with comfort. For this reason the sensitivities of the two types of plate are approximately the same. The greater number of events per unit area in the boron-plate is partly offset by the greater area examined in the lithium-plate.

Each batch of plates from the manufacturers is calibrated by exposure in the thermal column of the low-energy pile (GLEEP) in a flux previously determined by the indium foil method. Plates given an exposure of $1{\cdot}86 \times 10^8$ neutrons/cm.^{2} (corresponding to 0·26 of the tolerance two-week dose based on 80 hr. at 2,500 neutrons/cm.^{2}/sec. per 8-hr. working-day) yield mean event densities given in Table 2. From batch to batch there are differences in calibration of ± 10 per cent. The measured densities of events compare well with the values calculated from the loading data supplied by Messrs. Ilford. The thickness of the emulsion coating varies from plate to plate by up to 10 per cent, and corrections are applied based on the thickness of the processed emulsion layer. Boron-loaded emulsions are consistently of more uniform thickness than lithium-loaded emulsions.

Table 2. Plate calibration. Thickness, 50 μ. Exposure, $1{\cdot}86 \times 10^8$ neutrons per cm^2., that is, 0·26 of a fortnightly tolerance dose

Plate loading	Mean measured track density (Tracks per 10^{-4} cm.^{2})	Calculated track density (Tracks per 10^{-4} cm.^{2})
Boron	86	83·0
Lithium	7·25	8·36

A quite cursory examination of a plate yields information on the rough value of the dose, since for a boron-plate under a 2-mm. objective (× 95) a two-week tolerance dose gives 344 tracks per field (10^{-4} cm.^{2}) and for a lithium-plate under 4 mm. (× 44) 116 tracks per field (4×10^{-4} cm.^{2}). Examination of fifty fields, which can be done in half an hour, yields a dose determination, at this exposure-level, to a statistical accuracy of less than ± 1 per cent in the case of the boron-plates and ± 2 per cent in the case of the lithium-plates.

The detailed experiments indicate that an accuracy of 5 per cent can be expected without undue elaboration of measurement and calibration. From these results it appears that the method is accurate, convenient and economical in operation. Work on the allied, but more difficult, problem of fast neutron monitoring by a similar technique is in progress.

Miss M. Hall and Mr. T. A. Brinkley have helped in examining the plates. This note is published by permission of the Director of the Atomic Energy Research Establishment.

E. W. Titterton

Atomic Energy Research Establishment,
Harwell,
March 18.

[1] Macfarlane, A. S., *Nature*, **162**, 480 (1948). Capron, Faes and Tavernier, *Nature*, **163**, 129 (1949).
[2] Powell, Occhialini, Livesey and Chilton, *J. Sci. Instr.*, **22**, 102 (1946).
[3] Cheka, J. S., M.D.D.C. 890, *Phys. Rev.*, **71**, 836 (1947).
[4] Shapiro, M. M., and Barnes, J. R., *Phys. Rev.*, **73**, 1243 (1948). Titterton, E. W., A.E.R.E. Report *G/R240* (1948).

Transition Effect in Lead of Star-Producing Radiation

In a previous communication[1], we reported our measurements on the absorption at 3,500 m. above sea-level of star-producing radiation in lead and in other materials. The data from the absorption in lead, which were the most abundant of all, pointed to the existence of a transition effect with a maximum at about 20 gm./cm.^{2}. As this result, of particular interest in discussing the origin of the stars, was not in agreement with the results of other workers[2], we carried out some new observations so as to obtain better statistical data. Though our data are not yet completely collected, we can now affirm that the transition effect in lead really exists; and we wish to point out its particular property, namely, that it is produced almost entirely by the three-prong stars. This result is in agreement with some orientation measurements made by Amaldi and co-workers[3] with fast ionization chambers.

In the second and third columns of Table 1 we give the figures of the stars/cm.^{2}/day recorded in plates exposed at the Laboratorio della Testa Grigia.

Table 1

No. of prongs	Stars/cm.^{2}/day		
	Without absorber	Under 2 cm. of lead	Number expected under 2 cm. of lead
3	5·04 ± 0·28	6·80 ± 0·39	4·70
4	4·11 ± 0·25	4·60 ± 0·32	3·84
5	1·91 ± 0·17	1·89 ± 0·20	1·78
⩾6	3·07 ± 0·22	2·80 ± 0·25	2·85
Total	14·22 ± 0·48	16·10 ± 0·60	13·30

(3,500 m. above sea-level) without absorber and under 2 cm. lead respectively. For the correct evaluation of the transition effect the figures of the second column should be multiplied by the absorption factor for 2 cm. lead of the star-producing radiation, which is easily deduced from the mean path in lead of the same radiation. We then obtain, using the mean path of 29 cm. of lead from George and Jason[2], the figures of the fourth column in Table 1.

As we have not yet obtained data for materials other than lead, the effect is insufficient in itself to give us an idea about the secondary radiation responsible for it. But from many different considerations, which will be discussed in detail elsewhere, it seems reasonable to consider that this secondary radiation is generally composed of nucleons produced in the nuclear evaporations themselves.

With convenient mean angular distribution and mean multiplicity of the stars, it is possible to explain the rather small thickness where the maximum occurs; and at the same time, the rather low energy of the nucleons gives a reason for the difference of behaviour between stars with few prongs and those with many.

Table 2. Total numbers of stars found in the same Ilford *C*2 plate, 1 in. × 2 in., 100 microns thick, with two different scanning-rates

No. of prongs		3	4	5	6	7	8	9	10	11	12	Total
No. of stars observed for a scanning time of:	40 hr.	16	23	8	4	4	1	2	—	—	1	59
	60 hr.	37	36	10	4	5	1	2	—	—	1	96

To explain the difference between our results and the results of other authors, we must consider the following. First, the three-prong stars, which are responsible for the transition effect, fade much more easily than stars with many prongs; and we can say, from some orientation measurements, that this fading is not negligible and is strongly variable in different makes of the same type of emulsion. Secondly, the three-prong stars are easily missed as a consequence of any slight inconvenience in the scanning or developing. We purposely tested this, and we found that, on trying to speed up slightly the scanning time, there was a sensible diminution in the number of four-prong stars, and a great decrease in the number of three-prong stars, so that the graph of frequency of stars plotted against number of prongs showed a maximum for the four-prong stars. In Table 2, the figures are given for an Ilford *C*2 plate, 1 in. × 2 in., 100 microns thick, scanned twice at two different rates, corresponding to a time of about 40 hr. and 60 hr. respectively for the scanning of the whole plate. Moreover, it is constantly our experience that the check with high magnification (oil objective, × 100) of every star considerably affects the shape of the diagram of the frequency of the stars against number of prongs.

We are grateful to Prof. G. Bernardini for his help, and to Prof. E. Amaldi for a full discussion of the data.

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Rome.
April 5.

[1] Bernardini, G., Cortini, G., and Manfredini, A., *Nuovo Cimento*, **5**, 511 (1948); *Phys. Rev.*, **74**, 845 (1948).
[2] George, E. P., and Jason, A. C., private communication.
[3] Private communication.

Hyperfine Structure in the Solid State

A HYPERFINE structure has been observed in the microwave resonance-absorption band of a dilute Tutton salt, in which 95 per cent of the copper had been replaced by magnesium.

The resonance in this salt was investigated at a wave-length of 3·2 cm. in a magnetic field of about 3,000 oersted. The resonance band (due to reversal of the electronic spin in the magnetic field) was resolved into four lines, the distances between adjacent lines being about 93 oersted, when the magnetic field was directed along the K_1-axis. Evidently the applied field is sufficient to decouple the electronic from the nuclear spin, the four lines corresponding to the four possible orientations of the copper nucleus ($I = 3/2$) in the external field.

The two copper isotopes 63 and 65 have somewhat different magnetic moments[1]; but the corresponding sets of lines were not resolved.

Measurements were made with the magnetic field in various orientations in the a.c. plane. The overall structure varied by a factor of about 1·5, being a maximum along the K_1-axis, where the electronic g is a maximum, and a minimum near the K_2-axis, where the electronic g is a minimum.

Some time ago, L. J. F. Broer carried out calculations on the basis of Polder's[2] theory of the magnitude of the hyperfine structure in copper salts. The influence of hyperfine structure[3] had also been found experimentally by F. W. de Vrijer in the specific heat of the spin system of the same diluted copper salt. The absence of any observable hyperfine structure in the undiluted salt may be explained by the influence of magnetic interaction and exchange interaction between the copper ions. However, it is probable that the hyperfine structure contributes to the band-widths in copper salts[4].

R. P. Penrose

THE discovery described above was made by Dr. Penrose in the autumn of 1948. After his tragic death on April 28, 1949, I wrote this note, making use of a draft written by Dr. Penrose while in hospital.

C. J. Gorter

Kamerlingh Onnes Laboratorium,
Leyden.
May 12.

[1] Pound, R. V., *Phys. Rev.*, **73**, 745 (1947).
[2] Polder, D., *Physica*, **9**, 709 (1942).
[3] Gorter, C. J., *Physica*, **14**, 504 (1948).
[4] Bagguley, D. M. S., Bleaney, B., Griffiths, J. H. E., Penrose, R. P., and Plumpton, B. I., *Proc. Phys. Soc.*, **61**, 542 and 551 (1948). Bagguley, D. M. S., Griffiths, J. H. E., and Pryce, M. H. L. *Nature*, **162**, 538 (1948).

WE had the privilege of hearing of his results from Dr. Penrose shortly after his discovery of the hyperfine structure of paramagnetic resonance, and of discussing them with him when he was on a visit to Oxford. We were stimulated to make some calculations on the effect, in particular its variation with orientation of the applied field, and kept up a correspondence with him on this subject, which was interrupted by his death. The variation observed by Penrose differs markedly from the expected variation calculated by us on the basis of Polder's theory, and we believe that the discrepancy indicates an influence of the crystalline electric field on paramagnetic ions more complicated than has been generally assumed.

Penrose's work, therefore, opens a new and fruitful approach to the study of paramagnetism.

The hyperfine structure of paramagnetic resonances may readily be understood if one imagines that in a stationary state the ion exerts a magnetic field (He) on the nucleus. The nucleus takes up quantized orientations of different magnetic energies in this field. The transitions observed in the paramagnetic resonance of cupric salts, in which the ionic spin has the value $\frac{1}{2}$, correspond to a reversal of spin and consequently of He, the nuclear orientation remaining unchanged. The nuclear magnetic energy, therefore, changes sign, and there is a corresponding change in the quantum energy of the transition. The hyperfine splitting is clearly proportional to He, provided the external field is strong enough to decouple the ionic moment from the field of the nucleus.

The field He arises partly from the spin magnetic moment of the electrons and partly from their orbital motion. In Polder's theory, the ion is supposed to be in an electric field of tetragonal symmetry which partly quenches the orbital moment. On this basis we calculate that:

(1) When the external magnetic field is parallel to the tetragonal axis

$$He = \frac{\beta}{r^3}\left[-\frac{4}{7} + g_{\parallel} - 2\right] \text{ approximately.}$$

(2) When the external field is perpendicular to the tetragonal axis:

$$He = \frac{\beta}{r^3}\left[\frac{2}{7} + g_{\perp} - 2\right] \text{ approximately.}$$

In this expression small corrections which will not affect the following conclusions have been omitted. β is the Bohr magneton, and r is a suitable average radius of the $3d$ magnetic state of the cupric ion.

The observed g-values are roughly $g_{\parallel} = 2{\cdot}40$ and $g_{\perp} = 2{\cdot}10$, though there are definite deviations from tetragonal symmetry and the values differ slightly from salt to salt. This gives

$$\frac{(He)_{\parallel}}{(He)_{\perp}} = -\frac{0{\cdot}17}{0{\cdot}39},$$

from which it follows that the hyperfine splitting should be least along the tetragonal axis. Penrose's measurements were made with the field varying between the perpendicular direction and about halfway towards the parallel direction, and show a variation in the opposite sense.

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May 18.

Critical-Frequency Difference Variations and the Poynting Vector in the Ionosphere

Measurements of the F_2-layer critical-frequency difference, $f^x - f^o$, have now been made for the hourly records taken at Churchill, Manitoba, in 1945 and 1946 and at Portage la Prairie, Manitoba, in 1947 and 1948. These measurements corroborate results[1] already reported for Clyde River, Baffin Land, in that large diurnal variations in the apparent magnetic field are found. However, the calculation assumes that ordinary and extraordinary modes are reflected from the same region of the ionosphere.

The direction of energy flow has been calculated for the ordinary and extraordinary modes by considering the average Poynting vector. The total deflexion of the ordinary mode northward and of the extraordinary mode southward (in the northern hemisphere) has been found for different wave frequencies, latitudes and collision frequencies, assuming a parabolic distribution of ionization and vertical incidence.

It is found that both ordinary and extraordinary modes suffer a small westward deflexion when collision is taken into account.

The horizontal north–south distance separating the points of reflexion of the ordinary and extraordinary modes increases as the critical frequency is reduced, assuming a constant half-thickness of the parabolic region. But if a diurnal expansion and contraction of the F-region exists, a corresponding increase and decrease of the north and south deflexions will occur. The resulting diurnal variation in the horizontal distance between the points of reflexion of the ordinary and extraordinary modes, together with the normal gradient of ionization with latitude, furnishes a mechanism which can explain the measured variations in the $f^x - f^o$ critical frequency difference.

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Defence Research Board,
Ottawa.
Jan. 8.

[1] Scott, J. C. W., *Terr. Mag.*, **53** (June 1948).

Slope of the Ocean

For the component of steady flow at right angles to the pressure gradient on a rotating globe, the hydrodynamical equation may be written

$$u = -\frac{1}{2\rho\omega \sin\varphi} \cdot \frac{\partial p}{\partial y} + F, \qquad (1)$$

where u is the velocity of flow, $\partial p/\partial y$ the pressure gradient, φ latitude, ρ density, ω the angular velocity of rotation of the earth, and F any extraneous forces apart from pressure, for example, that due to wind friction at the surface of the ocean. In a paper on the effect of wind on ocean currents[1], I found empirically that the surface current due to wind flowed on the average at an angle of about 45° to the wind with a velocity $\frac{7{\cdot}9 \times 10^{-3}}{\sqrt{\sin\varphi}}$ that of the wind generating the current.

It is possible to divide the surface current, u, in equation (1) into two parts, u_1 due to the pressure gradient $\partial p/\partial y$, and u_2 due to the wind friction F. Then, from the relation above, allowance can be made for the wind friction, and so we get

$$u_1 = -\frac{1}{2\rho\omega \sin\varphi} \frac{\partial p}{\partial y}. \qquad (2)$$

This gives directly the pressure gradients in the ocean surface, from which can be deduced the slope of the ocean (related to a level surface) if we know the mean flow of the current and of the wind.

In the Marine Branch of the Meteorological Office, there are calculations of the vector mean wind flow and the vector mean surface currents for the oceans of the world for a long period of years which are eminently suitable for making such a calculation.

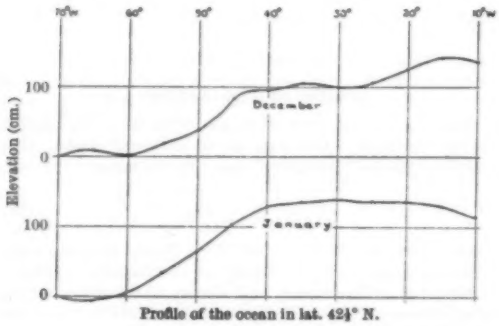
These records for December and January were used for preparing a profile of the ocean in lat. 42½° N. from long. 10° W. to long. 70° W. In these calculations allowance had to be made for the north–south flow of air as well as the east–west. Now in this latitude a difference of 1 mb. or (1 cm. of water-level) at a distance of 5° of longitude creates a flow of $4{\cdot}82 \times 10^{-3}$ knots, by equation (2). So we can get the following table to illustrate the difference in pressure in the sea surface which is necessary to produce the residual currents.

Mean pressure profile in the sea surface, mean surface atmospheric pressure and mean elevation of the surface of the ocean in lat. 42½° N.

Longitude (W.)	70° W.	65°	60°	55°	50°	45°	40°	35°	30°	25°	20°	15°	
December :													
Pressure profile (mb.)	0	8	−2	17	35	81	95	106	100	108	131	145	143
Surface pressure (mb.)	1016	1014	1013	1013	1013	1014	1016	1017	1018	1019	1020	1020	1022
Elevation of surface (cm.)	0	8	1	20	30	83	95	105	98	105	127	141	137
January :													
Pressure profile (mb.)	0	−10	2	31	58	100	130	138	141	138	138	129	118
Surface pressure (mb.)	1015	1014	1012	1012	1012	1014	1015	1016	1016	1017	1018	1019	1020
Elevation of surface (cm.)	0	−9	5	34	61	101	130	137	140	136	135	128	113

The last lines of this table have been plotted in the accompanying figure, which gives the elevational profile across the Atlantic.

The conclusion is that in winter the surface of the ocean is nearly level from off the coast of Spain to the longitude of 40° W. From that longitude it slopes downwards to long. 65° W., the fall amounting to a metre.



Profile of the ocean in lat. 42½° N.

It is unfortunate that this method of calculating the profile of the ocean cannot be used right up to the coast-line unless very precise information is available regarding wind and current, because the local variation of wind and the streaming of current due to the slope produced by the coastal boundary are both of considerable importance. The method also cannot be confidently used in the tropics, because the accelerational terms in the hydrodynamical equations are of major importance far more frequently in the tropics than in extratropical latitudes. It would, however, be possible from the data which has been assembled in the Marine Branch to draw seasonal contour charts of the oceans in extratropical regions.

My thanks are due to the Director, Meteorological Office, for permission to publish these notes, based on the data provided by the Marine Branch.

C. S. Durst

Meteorological Office,
Air Ministry,
Kingsway,
London, W.C.2.

[1] *Quart. J. Roy. Met. Soc.* **50**, 113 (1924).

Limit of Useful Amplification of a Galvanometer Deflexion

The following observations may be of interest in connexion with the demonstration of a short-period galvanometer by Attree, Downing and myself at the recent Physical Society Exhibition. The extent of useful amplification of this instrument has since been tested at University College on an ordinary afternoon : the limit is set by the disturbances, external and intrinsic, to which it is liable. To measure these disturbances the galvanometer was set up for photoelectric amplification and cathode-ray display, and its sensitivity increased to $2{\cdot}4 \times 10^{-10}$ amp./mm., which is equivalent to a scale distance of 370 m. for direct reading. The period on open circuit was 0·0125 sec., the internal resistance 13 Ω, the resistance (total) for critical damping 230 Ω. With an external resistance of 90 Ω, single sweeps were recorded, each consisting of 50–60 'instantaneous' spots at 0·02 sec. intervals. Eight such sweeps are shown in Fig. 1; they illustrate the basic unsteadiness at extreme sensitivity.

Twenty sweeps were recorded on photographic plates at intervals during twenty minutes. The records were projected (× 3·56) by a lantern on to squared paper, a line on the paper being adjusted by eye to run through the middle of the 50–60 spots in each trace, the spots being marked in by pencil. Altogether 1,076 spots were so marked, none being omitted. Their deviations from the mean lines were measured to 0·1 mm. and a distribution curve constructed. In Fig. 2 each small circle gives the observed number of deviations of magnitude equal to or less than the abscissa. The curve was calculated from the error function, and the agreement shows that the distribution is normal. The r.m.s. value of the deviation is 1·47 mm., corresponding to 0·41 mm. on the original record, or $1{\cdot}0 \times 10^{-10}$ amp.

The Brownian movements of a galvanometer system can be calculated to give an r.m.s. deflexion equivalent (at 17° C.) to $1{\cdot}12 \times 10^{-10}/\sqrt{(rt)}$ amp. in

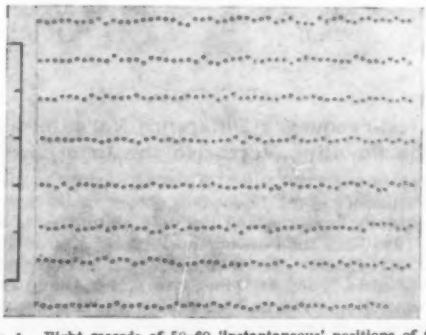


Fig. 1. Eight records of 50–60 'instantaneous' positions of the galvanometer trace every 0·02 sec. Sensitivity of galvanometer 1 mm. = $2{\cdot}4 \times 10^{-10}$ A. Scale marked at side in cm. Mean fluctuation 0·41 mm. ≡ $1{\cdot}0 \times 10^{-10}$ A.

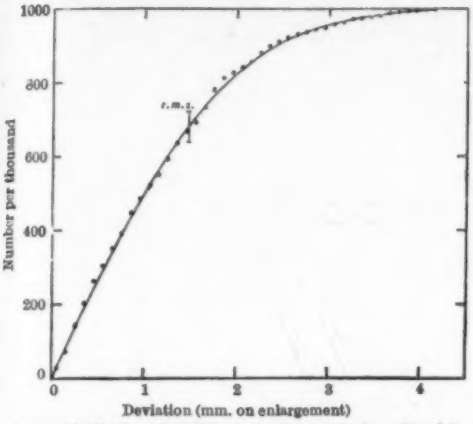


Fig. 2. Distribution of random fluctuations. Abscissa, size of deviation, mm. on enlarged (× 3·56) record: ordinate, observed number (per thousand) of 'spots' with deviation equal to or less than the abscissa. Curve drawn from the error function. The r.m.s. value of the deviation is marked

the galvanometer, where r is the total resistance for critical damping and t the period on open circuit. Inserting the values of r and t, the R.M.S. value is $0{\cdot}66 \times 10^{-10}$ amp. The observed mean deviation is 1·5 times this. If, therefore, all extraneous disturbances could be eliminated, the fluctuations observed would be reduced by one third: or, since random disturbances are compounded by squaring, adding and taking the square root, the extraneous and the intrinsic disturbances are about equally responsible for the observed unsteadiness. Under quiet conditions in the early morning, external vibrations would be less and a close approach might be made to the Brownian movements alone.

The mean unsteadiness of this galvanometer on its anti-vibration carriage would correspond to 1-mm. fluctuation if the reflected beam could be read directly on a scale about half a mile away: it is equivalent to a mean fluctuation of about 0·4 μ on the photocell.

The galvanometer is normally intended to be used with negative feed-back from the head-amplifier, diminishing its sensitivity, period and critical damping resistance as required. The same treatment can be applied under such conditions, with the appropriate values of these quantities.

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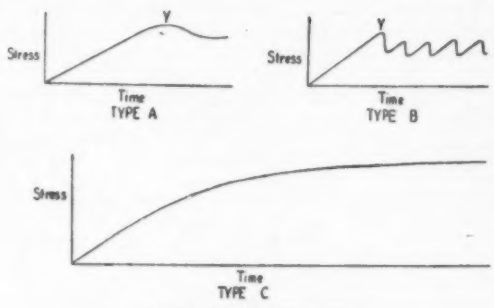
Yield Value of Bentonite Suspensions

I have recently been measuring the yield values of thixotropic aqueous suspensions of bentonite by determining the force required to move a thin flat plate in its own plane, through the suspension[1-3]. The results indicate that the yield value, as measured, cannot be regarded as an intrinsic property of the suspension but depends to a very marked extent on the material of the plate. The plate (3 cm. × 2·5 cm. × 0·008 cm.), immersed in the suspension, is suspended from a horizontal arm attached rigidly to a torsion wire. The vessel containing the suspension is slowly lowered (0·18 cm./min.) and the stress on the plate measured by the deflexion, recorded photographically as a function of time, of the horizontal arm. The suspensions (*p*H *c.* 8·5) are aged for at least a month after mixing and are thoroughly dispersed by a high-speed stirrer immediately before each experiment. The time of repose, until the yield point is reached, is approximately constant (*c.* 4 min.).



Traces of stress time curve

With various bentonite suspensions three types of trace are obtained, as shown in the accompanying graphs. In types *A* and *B* the suspension is behaving elastically almost up to the yield point *Y*, and thus the slope of the first portion of these curves gives an indication of the rigidity of the gel. The differences in behaviour after the yield point can be explained by differences in the rate of thixotropic recovery in relation to the mechanical constants of the apparatus. In type *C* trace there is apparently neither a true yield value nor truly elastic behaviour.

Variation in yield value with plate material

Suspension	Plate material*	No. of observations	Average yield value	Standard error of average yield value
5% Wyoming bentonite, *p*H 8·4	aluminium	10	40·8	1·4
	mica	10	30·6	0·7
	paraffin wax (on aluminium)	8	20·9	0·7
5·5% Wyoming bentonite, *p*H 8·4	mica	7	55·0	3·1
	platinum	7	31·3	2·2
5% Wyoming bentonite, *p*H 8·4	mica	6	27·7	0·6
	platinum + $AlCl_3$	6	44·0	2·0
6·1% Queensland bentonite (fractionated to particle size < 1 μ), *p*H 8·5	aluminium	6	76·2	1·9
	platinum	6	57·7	3·6
	platinized platinum	6	27·2	3·8
6% Queensland bentonite (fractionated to particle size < 1 μ), *p*H 8·5	electro-polished aluminium	5	70·2	3·6
	rolled aluminium	4	74·5	2·9
	serrated† aluminium	6	80·9	1·4

* Except where otherwise stated, the metal plates had the normal commercial (rolled) finish. After an initial degreasing, the detergent nature of the bentonite suspensions themselves kept the plates clean.
† By gripping in the serrated jaws of a vice.

The yield values obtained with various plate materials are given in the table. It will be seen that the order of the apparent yield values for the three plate materials used is aluminium > mica > platinum. This could possibly be due to differences in surface finish, or in contact angle, or to a physico-chemical

interaction between the plate and the suspension. Surface finish, however, cannot be the major factor since plates of aluminium with widely differing surface finish give yield values that are not significantly different. Nor can the effect be due to variation in contact angle since, although the yield value can be drastically reduced by making one plate hydrophobic by coating with paraffin wax (or silicone), all the other plates are completely wetted by the suspension. In fact, the platinized platinum surface which is completely wetted, and certainly not smooth, gives, relatively, the lowest yield value.

That the high yield values obtained with the aluminium may be due to interaction between the plate and the suspension is indicated by the experiment in which a platinum plate, covered with a thin film of aluminium chloride (by dipping in a hot solution of the salt and allowing it to dry), gives practically the same yield value as does the aluminium. There is the further fact that if an aluminium plate is negatively charged (1 volt) the apparent yield value is decreased, and *vice versa*. This latter effect, of course, could be due to electrophoresis of the negatively charged bentonite particles. Whatever the mechanism by which the aluminium affects the yield value, the effect would seem to be purely a surface one, since the yield value as measured with a mica plate is not significantly altered by additions of aluminium chloride to the bulk of the suspension, although the rigidity is sharply reduced.

Thus it would seem that though the true explanation of the effect is still obscure, any method of measuring the yield value of non-Newtonian liquids must take into account the materials of which the measuring instrument is constructed.

This work, which is being continued, was carried out during an investigation of the properties of Australian bentonites and forms part of the general programme of research of the Division of Tribophysics of the Council for Scientific and Industrial Research, Australia.

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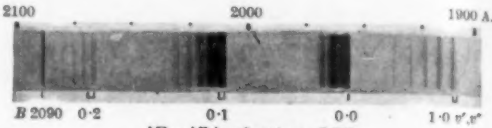
Dec. 29.

[1] Veiler, S. J., and Rehbinder, P. A., *C.R. (Doklady), U.R.S.S.*, **49**, 345 (1945).
[2] Mardles, E. W. J., *Nature*, **158**, 199 (1946).
[3] Metrot, R., *Rev. Inst. Fr. du Petrole*, **1**, 78 (1946).

New Electronic Band-Systems of Diatomic Boron Compounds (BF, BO and BH)

As is well known in the series of the isoelectronic molecules N_2, CO and BF, nitrogen and carbon monoxide show a close similarity not only in their chemical but also in their spectroscopic properties, that is, in the structure of their excited states. The ground-state of both N_2 and CO is saturated ($^1\Sigma$) and both have systems of analogous singlet and triplet levels. However, the data hitherto recorded of BF, the least symmetrical of the above-mentioned molecules, show no evident similarity, and therefore it has been suggested in the tables of Herzberg and of Gaydon[1] that the ground-state of BF had escaped observation.

Continuing experiments which some time ago[2] gave strong band-systems ($^1\Pi - {}^1\Sigma$) of BCl and BBr, we have now found the main system of BF situated in the near Schumann region (2100–1900 A.). As light source we used a discharge in circulating helium at about 10 mm. mercury pressure, into which we introduced boron fluoride gas (BF_3). The vacuum spectrograph (1-m. grating with 30,000 lines/in.) was separated from the discharge by a thin lithium fluoride window.



$^1\Pi - {}^1\Sigma$ band-system of BF

The new $A^1\Pi - X^1\Sigma$ system of BF is extremely intense (1 min. exposure time on Ilford *Q* I plates), showing prominent group structure and well-marked isotope separations $B^{11}F - B^{10}F$. The bands are degraded to the red. However, their rotational structure could not be analysed with the dispersion used (8·3 A./mm.)*. The accompanying table contains the constants, in cm.$^{-1}$, of the BF-terms calculated from about forty measured *Q*-heads together with analogous data of CO.

Term		BF			CO		
		A_e	ω_e	$\omega_e x_e$	A_e	ω_e	$\omega_e x_e$
D		72291	—	—	92923	2134	—
C	$^1\Sigma$	69113	1610	(15)	91923	2133	—
B	$^1\Sigma$	65483	1687	(10)	86920	2182	50
A	$^1\Pi$	51083	1265	14	64756	1515	17
X	$^1\Sigma$	0	1400	12	0	2167	13

At shorter wave-lengths there are present on the plates weaker violet-degraded band systems classified as $B - X$ (1630–1480 A.) and $C - X$ (1520–1400 A.). Both contain the fundamental frequency $\omega'' = 1400$ cm.$^{-1}$ and isotope doublets checking their assignment to BF. A further band at the end of the wave-length range used may be the (0,0)-band of a system $D - X$.

The $A - X$ system corresponds obviously to the main system ('4th positive') of CO, the $B - X$, $C - X$ and $D - X$ systems to the Hopfield – Birge bands. The triplet terms of BF giving rise to the known spectra in the near ultra-violet and visible region have not yet been connected with the new singlet states, but doubtless belong to excited levels of the molecule in accordance with the results on the other isoelectronic members.

With increased current, owing to the reaction of the halide vapours with the glass wall of the discharge tube, new bands of BO appeared in the Schumann region, the wave-lengths and intensities of the most prominent being 1835 (2), 1774 (5), 1735 (4), 1717 (0), 1699 (2) and 1682 (1) A., with the difference 1850 cm.$^{-1}$, that is, the fundamental frequency of BO. They are similar in appearance to the known BO-bands, but evidently belong to new higher excited states of the molecule.

Further, when hydrogen is added to a mixture of boron bromide (BBr_3) vapour and helium, we observed a new band at 1920 A. coinciding with the hitherto undetected $C - A$-band of BH (Douglas[3]).

Details of the new spectra will be published in *Helv. Phys. Acta*.

* *Note added in proof* (*May* 24). We have now succeeded in getting second-order spectrograms of the

BF-bands which permitted a partial analysis of the rotational structure.

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Jan. 4.

[1] Herzberg, "Molekülspektren" (1939). Gaydon, "Dissociation Energies" (1947).
[2] Miescher, *Helv. Phys. Acta*, **8**, 279 (1935).
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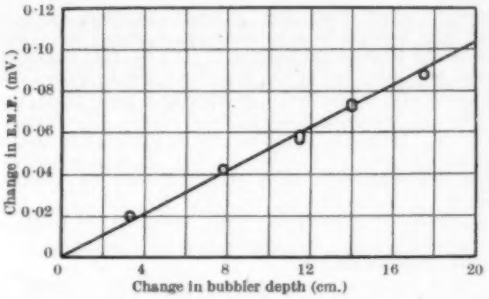
The Hydrogen Electrode

The hydrogen electrode is the primary standard to which all electrode potentials are referred and is used in cells without liquid junction for measurements of high accuracy. The potential of the hydrogen electrode is corrected to the standard pressure of one atmosphere on the basis that the electrode is in equilibrium with the measured partial pressure of hydrogen above the solution. It is, however, not the gas phase which directly determines the potential, but the hydrogen in solution; and a tacit assumption that the concentration of dissolved hydrogen is unequivocally defined by the gas pressure is false. This error can arise in two ways.

(1) In most hydrogen half-cells, the gas is delivered below the surface of the solution by means of a jet. The pressure in the rising bubbles is greater than that over the surface because of hydrostatic pressure, and the concentration of dissolved hydrogen rises to a value exceeding that in equilibrium with the measured gas pressure. This supersaturation is not rapidly dispersed on cessation of bubbling but persists in the still solution for many hours. The degree of mixing achieved by reasonable bubbling-rates ensures homogeneity of the solution, so that the potential of the electrode is independent of its depth of immersion, but is strongly dependent on the depth of the bubbling jet.

Correction for this effect has been made only on an empirical basis, since its exact calculation would involve a knowledge of the motion of large bubbles and would require a mathematical treatment of turbulent flow of some complexity. On the assumptions, however, that the free surface of the solution is ineffective in relieving supersaturation and that the velocity of the rising bubbles is approximately constant, it can readily be shown that the effective 'excess pressure' over the surface pressure is close to one half of the hydrostatic head of the solution above the jet. Thus, for a bubbler of 10-cm. depth, the solution would be supersaturated to the extent of 5 cm. of water pressure, and the electrode would be 0·062 mV. more negative than a similar electrode operating at the surface pressure.

We have carried out experiments to determine this correction, using a cell consisting of two normal hydrogen electrodes in separate electrode vessels. One vessel contained a bubbler of constant depth; the other contained a fixed bubbler at the bottom of the vessel and a concentric dipping bubbler, the depth of which was varied in each experiment. The E.M.F. of this cell was measured with hydrogen passing alternately through the fixed and dipping bubblers of the second vessel. The results, shown in the accompanying graph, indicated a very systematic dependence of electrode potential on bubbler depth. From the slope of this plot the excess pressure



correction (expressed as a factor of the total head above the jet) was 0·42 ± 0·02. Variation of this factor with apparatus design is likely to be small and is of second order in importance.

The effect of the free surface of the solution in relieving supersaturation is, in fact, slight. This has been shown in two ways. First, a half-cell was saturated with hydrogen under increased pressure. On releasing the pressure with simultaneous cessation of bubbling, the excess negative potential persisted for many hours: if, however, bubbling was resumed at the lower pressure, the electrode responded immediately and its negative potential fell to its appropriate value. A corresponding increase in applied pressure without bubbling produced no effect. Secondly, a half-cell was examined with hydrogen bubbling through the solution and pure nitrogen sweeping over the surface: the divergence of potential from normal was only 2·7 mV.

(2) An error in the opposite sense can occur in a cell without liquid junction by failure to attain or maintain the maximum concentration of dissolved hydrogen, which tends to diffuse away from the electrode vessel. Such deficiencies can readily occur by opening a connexion between the hydrogen half-cell and the other electrode vessel, and in ill-designed apparatus containing bulks of solution not properly swept out by hydrogen gas bubbles.

These factors, which do not appear to have been described in the literature, point to the necessity for studying the concentration of dissolved hydrogen as carefully as that of any other solute. Existing hydrogen electrode measurements might be corrected to zero bubbler depth, where apparatus dimensions are available, and this would cause a shift (0·02–0·08 mV.) in most of the accurate electrode potentials which have been measured against the hydrogen electrode.

It is intended to report these experiments in detail, in connexion with other work, in due course.

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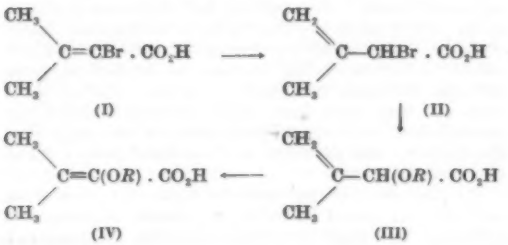
Anionotropic and Prototropic Changes in the α-Bromo-ββ-dimethylacrylic Acid System

It has already been shown by one of us[1] that treatment of α-bromo-ββ-dimethylacrylic acid (I) with sodium alkoxides in the corresponding alcoholic solution gives a mixture of α-alkoxy-ββ-dimethylacrylic acid (IV) and α-alkoxy-β-methylenebutyric acid (III), with the latter preponderating. As a result of more detailed investigations it has now

become clear that the 'normal' product (IV) is actually formed by a prototropic change from (III), since the relative proportion of (IV) to (III) increases with the duration of the reaction; furthermore, the former can be prepared in excellent yield from the latter by warming with aqueous alkali. The formation of (III) can be explained by an initial prototropic rearrangement of the bromo-acid (I) into α-bromo-β-methylenebutyric acid (II), followed by nucleophilic replacement of bromine by alkoxyl; the reactivity of the bromine atom, which in the original compound would be expected to be non-reactive, is thus understandable.

This prototropic change is of particular interest in view of the fact that ββ-dimethylacrylic acid itself cannot be induced to undergo isomerization to β-methylenebutyric acid[2], and it therefore appears that the presence of the bromine atom in the α-position exerts a marked effect on the αβ : βγ-isomerization. It may be significant in this connexion that hyperconjugation is said[3] to exist in the system C=C—C—Br; such an effect in the case under consideration might be expected to stabilize the double bond in the βγ-position.



There is, however, a further consequence of the above isomerization. The bromo-acid (II) is clearly an anionotropic system, and might, therefore, be expected to undergo not only a normal replacement of bromine but also a replacement with rearrangement, which would give rise to the formation of γ-substituted ββ-dimethylacrylic acids (V). In the reactions with alkoxides, no such γ-substituted products were observed, the conditions being such as to favour nucleophilic replacement without appreciable rearrangement; but since it is recognized that rearrangement in anionotropic systems is facilitated by solvents of high dielectric constant, we proceeded to investigate the reaction of α-bromo-ββ-dimethylacrylic acid with aqueous sodium hydroxide. The following products were obtained: α-keto*iso*valeric acid [keto-form of α-hydroxy-ββ-dimethylacrylic acid (IV; $R = H$)]; α-hydroxy-β-methylenebutyric acid (III; $R=H$); β-methyl-γ-hydroxycrotonic acid (V; $R=H$). The structure of (III; $R=H$) was confirmed by hydrogenation to α-hydroxy*iso*valeric acid and by ozonolysis to formaldehyde and acetol; on prolonged treatment with alkali it underwent prototropic change into α-keto*iso*valeric acid. The structure of (V; $R=H$) was proved by ozonolysis to acetol and by hydrogenation to γ-hydroxy*iso*valeric acid (isolated as the known γ-lactone, and giving methylsuccinic acid on oxidation). The results are thus in complete agreement with the suggested mechanism.

So far as we are aware, these experiments afford the first recorded examples of anionotropic and prototropic changes in the reactions of unsaturated bromo-acids. Full details of this work, and a more comprehensive consideration of its theoretical implications, will be published elsewhere.

One of the authors (M. U. S. S.) wishes to thank the Ceylon Government for the award of a postgraduate scholarship.

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Dec. 17.

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[3] de la Mare, P. B. D., Hughes, E. D., and Ingold, C. K., *J. Chem. Soc.*, 21 (1948).

Lanthanon (Rare-Earth) Sodium Sulphate Precipitations

In addition to using sodium sulphate in the classical manner to effect a separation of the light (cerium group) lanthanons, leaving the heavy group and yttrium in solution, I have found it advantageous to precipitate the whole of the 'soluble' double sulphates. This is achieved (except for the equivalent of 0·2 gm. per l. of oxide) simply by salting out. In hot brine, solubilities of the yttrium group are reduced to less than 1 per cent of the minimum obtainable in practice with excess of sodium sulphate in hot aqueous solution.

The use of brine is particularly advantageous in fractionating middle (gadolinium) and heavy (yttrium) lanthanon mixtures. It permits of the use of more dilute solutions with a consequent gain in selectivity and control. It also reduces the consumption of sodium sulphate.

To the dilute lanthanon solution in concentrated brine a suitable quantity of sodium sulphate is added at 35° and the temperature slowly raised while the solution is well stirred. If not too much sodium sulphate is added, the precipitation of double sulphate will take place slowly with increasing temperature and be highly selective. Some five or six fractions may be taken thus before all the lanthanon is fully precipitated at boiling point. The precipitates are reconverted to chlorides via the hydroxides, again added serially to the brine, and another set of double sulphate fractions obtained. In this way a rapid separation is achieved. Although the solubility of sodium sulphate in brine is comparatively small[1], in presence of lanthanon only double salt will precipitate.

I confirm the observation of Bettendorf[2], who used sodium sulphate to separate the middle lanthanons from yttrium, that a rapid separation of holmium and erbium is also achieved. Moller and Kremers[3] more recently were not successful in getting these two to separate appreciably.

When double sulphate precipitates are nearly free from dysprosium and earlier lanthanons, if desiccated at 400° and ground to 20 mesh, they dissolve readily (400 gm./l.) in ice water. This process presents an easy means of fractionating the more soluble double sulphates; but its merits relative to basicity processes still require determination.

The double sulphate process is the oldest known for separating the lanthanons. Owing, however, to

the existence of obstinate metastable phases its study has given no satisfaction[4]. No other fractionation process offers a comparable separation in an equal number of operations. New techniques may give improved final purifications; but the importance of good preliminary methods of obtaining concentrates of individual lanthanons is not reduced. For this purpose the use of double sulphates appears capable of further exploitation, even after a hundred and fifty years of use.

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[1] Int. Crit. Tables, **4**, 280, 287.
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Quantitative Inorganic Paper Chromatography: Sub-micro Separation and Determination of Aluminium, Iron and Titanium

PAPER chromatography of cations and anions in aqueous solution has so far been very little investigated[1]. Quantitative determinations have been achieved by R. P. Linstead and co-workers, who have already obtained very important results. Our present work began in 1946, and it appeared that organic solvents behave in inorganic paper chromatography as they do in organic paper chromatography. The adsorbed salt spot gives rise to a chromatogram depending from the cation and ion of the salt, the *p*H of its solution, the nature and the *p*H of the developing organic solvent. Changes in the composition of this solvent are likely to alter the chromatogram just as in organic chromatography[2]. Our experiments have dealt only with iron, titanium and aluminium chlorides, cobalt and nickel chlorides and nitrates, using downward development.

The salt solution containing 1–10 γ is taken directly from a horizontal capillary burette (0·01 ml. $\pm$ 0·0002 ml.) and deposited at the usual position (3·5 cm.) from the top of the strip of Whatman No. 1 filter paper. So far, only water-soluble solvents have given satisfactory separation. This does not mean that it will not be possible to use other solvents. Pure ether oxides cannot be used. Dioxane, in the dark, takes the iron to the liquid front line. Alcohols have a decreasing developing effect with increasing molecular weight, whereas for aliphatic acids (formic and acetic) the reverse is the case. Acetone brings iron also to the front line.

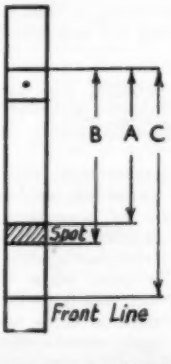
Acid salt solutions have different chromatograms from neutral ones, even if the liquid spot is dried before proceeding to development. This is evidence of the interference of the paper elements in this chromatographic process. Another proof of this effect is that the result is also different from the chromatogram obtained with an acid-developing solvent containing a percentage of acid equivalent to that contained in the drop of salt solution.

Chlorides and nitrates are liable to give different results, chlorides being preferable for separations. Mixed solvents like pyridine and acetone, pyridine and methyl alcohol, pyridine and water are good developing solvents for our cation mixture, giving sharp, well-separated spots, that is, iron and titanium from aluminium and vanadium. Formic acid and alcohol have also been successfully used.

Addition of dry or concentrated hydrochloric acid or ammonia to a solvent is apt to improve the sharpness of the spots. One example of this can be cited with acetone and alcohol mixtures on cobalt and nickel chromatography. 1 per cent of hydrochloric acid added to this solvent brings about a very good separation of both metals, cobalt lying on the front line, whereas in absence of the acid there is no separation at all. These separations take no longer than 30–60 minutes.

Dilute formic acid can also be used for the quantitative separation of aluminium from titanium and iron. This solvent action has another aspect, in that small changes in their composition cause appreciable differences in chromatograms. That property can be used to detect impurities in solvents. For example, pure dioxane gives a sharp front line of iron; when this solvent contains peroxide, iron spots are dispersed and the difference is perceptible with 2 per cent of peroxide impurity in the dioxane. Variation of the percentage of pyridine in water can bring about the reversion of the iron–titanium–aluminium chromatogram. There is certainly much to be learned from study of mixed solvents in inorganic chromatography.

For qualitative purposes, the paper strip bearing the spots is dried after development and sprayed with the appropriate reagent or left for a time in a saturated atmosphere of the reagent (hydrogen sulphide or ammonia).



For quantitative purposes, the developing process is no longer necessary. Measurements of A, B and C on separate strips serve as guide to locate the spots on the undeveloped strips. A/C and B/C are characteristics for each element in the experimental conditions chosen. For example, 6·5 N formic acid after 60 min. gives the following:

		Iron	Titanium	Aluminium
A	=	80 mm.	72 mm.	100 mm.
B	=	91 mm.	92 mm.	106 mm.
C	=	100 mm.		

showing that separation in this case is possible for aluminium.

After the measurements have been made, the dried strip is cut up accordingly and each part can be eluted for quantitative determination. This method has been used to separate aluminium from titanium and iron in acid solution (*p*H 0·7). As above described,

it can be used for 5–10 γ of aluminium contained in 0·01 ml. of solution. One developing solvent used, among others, is formic acid. After 60 min. of development at 20° C., aluminium lies on the front line and the two other elements above. Drying and cutting could easily be done now; but the chances of quantitative separation can be improved by increasing the distance between the aluminium spot and the iron–titanium one. Air saturated with water vapour (13 2 mm. at 20° C.) produces a migration of the aluminium spot down towards the front line, and in 100–135 min. the bottom part of the aluminium spot reaches about 40 mm. from the initial front line, the iron–titanium spot remaining above it. Drying again and cutting the paper along the first front line is all that has to be done before elution.

Aluminium is quantitatively eluted by water at 80° C.; it is not eluted satisfactorily by acid solutions, which demonstrates again the interference of the paper in this process.

Simple soaking in 2 ml. of redistilled water for 40 min. brings all the aluminium into solution. The same procedure with mixtures of acetic and hydrochloric acid brings the iron–titanium into solution.

Spectrophotometric determinations of aluminium by aluminon gave for

10 γ: pH = 4·7; λ = 520 μm. 10·0, 9·8, 10·1, 9·7, 10·1, 10·2 γ
9 γ: pH = 4·7; λ = 520 μm. 8·8, 9·0, 9·3, 9·4, 9·4, 9·4 γ
Blanks are only 1 in 190 (density)

A better elution than by soaking is obtained for iron and titanium by siphoning a 1 per cent solution of hydrochloric or sulphuric acid through the paper strip for 1½ hr., according to Martin's technique.

Iron and titanium are determined in one solution by tiron[3]:

(Fe) pH = 4·7 λ = 560 mm. for 10 γ: 10·3, 10·3, 10·0, 10·0 γ
Blanks are 1 in 32 (density)
(Ti) pH = 4·7 λ = 410 mm. for 10 γ: 10·3, 9·8, 9·9, 9·6 γ
Blanks are 1 in 90 (density)

The whole procedure with formic acid is rather longer than with other solvents on account of the capillary migration of aluminium. But a great number of determinations can be carried on at the same time, and spectrophotometric determinations of aluminium can be done while iron and titanium elutions are going on. We hope to achieve the whole quantitative operation in three hours, including the chromatography and development with solvents necessitating no migration for aluminium. Quantitative separation of nickel and cobalt is being done, and similar work with the separation of other cations and anions.

We are indebted to the I.R.S.I.A. for financial help for this work.

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Action of Thiosulphate on the Kidney of the Cat

Sodium thiosulphate has recently been recommended as a substance suitable for use in measuring the glomerular filtration-rate (G.F.R.) in the kidney. Its concentration in blood and urine is easily and reliably measured, it is neither re-absorbed nor secreted by the tubule cells in man[1] or dog[2,3], and is stated to be non-toxic. In the cat it behaves very differently. It is actively secreted, that is, its clearance is higher than that of creatinine, which is a measure of the glomerular filtration-rate, and its clearance rises with falling concentration in the plasma, as can be seen in Fig. 1. This finding confirms the recent suggestion of Bing and Effersoe[4].

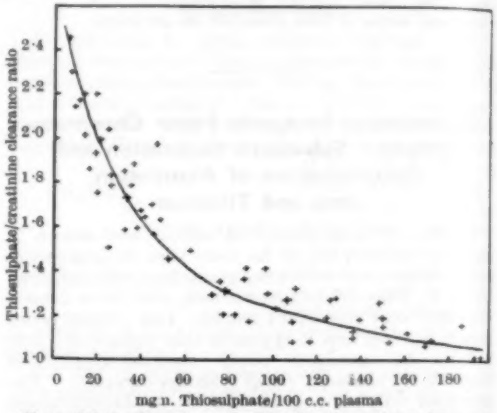

Fig. 1. Relationship between the thiosulphate/creatinine clearance ratio and thiosulphate concentration in the plasma of cats

A second effect of thiosulphate which these authors failed to note, probably because the substance was always injected at the same time as the creatinine, is a strongly depressant action on the kidney itself. The susceptibility of individual animals to this action varies widely, and the results of an experiment shown in Fig. 2 demonstrate how great this may be.

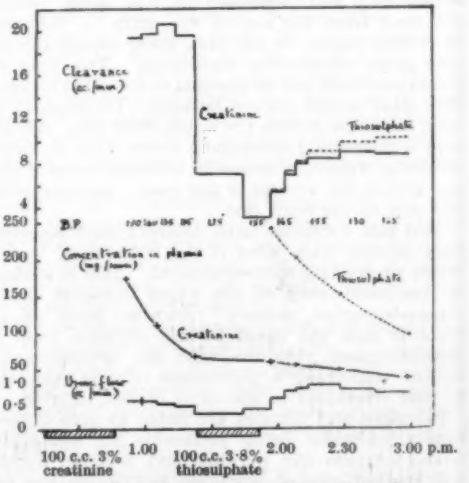

Fig. 2. Action of thiosulphate in depressing creatinine clearance. Cat 3·5 kgm. Nembutal anæsthesia

The glomerular filtration-rate was reduced to less than a fifth of its original value, and after some degree of early recovery remained thereafter well below half its original value. Smaller doses of the substance produce a less dramatic fall; but on one occasion 2 gm. in a 6-kgm. cat, which is equivalent, weight for weight, to the dosage used in man, caused a 50 per cent depression in glomerular filtration-rate.

Newman *et al.*[1], as a result of their investigation on man, conclude that "preliminary observations show that injections of thiosulphate cause no change in inulin clearance (G.F.R.)", but continue cautiously, "more studies are necessary to establish this beyond question". Although the cat's kidney may be more susceptible than that of man to the observed depressant action of thiosulphate, the results presented here strongly support the suggestion that more studies should be made on man before this substance comes into general use in the measurement of glomerular filtration-rate.

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Jan. 25.

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Intermittent Periosteal Activity

As part of a study of bone-formation under various pathological conditions, I have used a method of fine-detail tissue radiography similar to that developed by Barclay[1] for the study of renal vascular architecture. In conjunction with the ordinary methods of histological study, the demonstration of the distribution of bone salt by this method has been of great service in obtaining a comprehensive picture of the processes involved in structural changes in bone.

On several occasions, in regions of recent periosteal bone-formation, radiographs of this sort have been found to show a number of zones, demarcated from each other by lines where the direction of the bone trabeculæ is suddenly altered, although their continuity is not broken. The appearances suggest that these lines, which are smoothly contoured, mark successive positions of the bone-forming periosteal layer at periods of its temporary inactivity, while the intervening zones correspond with periods of longitudinal growth of the bone trabeculæ in the region concerned.

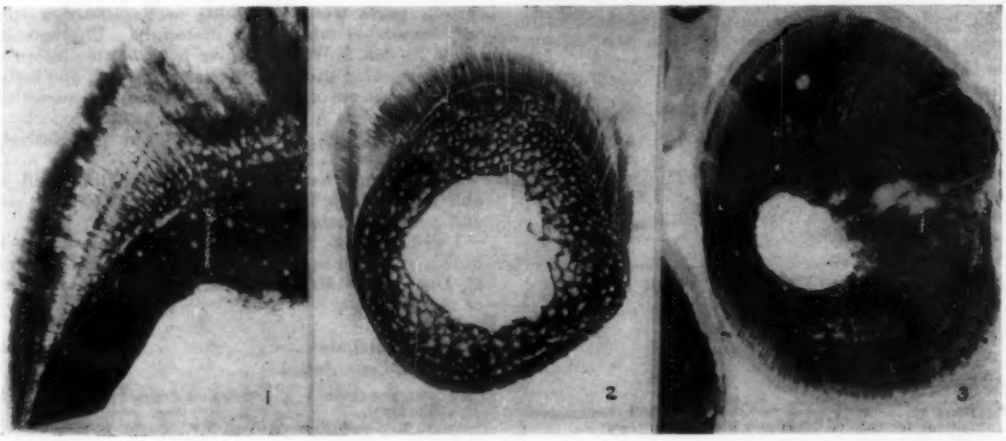
These zones of intermittent periosteal bone-formation were first noticed in circumstances where intermittent extrinsic factors (such as repeated doses of therapeutic irradiation) might have been responsible for their presence. The present illustrations, however, are from lesions where such factors can be excluded.

Figs. 1 and 2 are radiographs of tissue-slabs in regions of reactive periosteal bone-formation in the neighbourhood of primary bone sarcomas. Fig. 3 is a radiograph of a transverse section through a bone presenting the changes of virus-produced avian 'osteopetrosis'. The zones and lines described can be seen in each of the illustrations in the superficial newly formed bone. Each of the tissue-slabs is approximately 1·5 mm. in thickness, and the radiographs are enlarged to three times their actual size.

In the deep part of the subperiosteal bone shown in Figs. 1 and 2, extensive rearrangement of the originally radially directed trabeculæ has taken place, but traces of the transverse linear periosteal markings are still apparent.

Because of their extreme thinness, ordinary histological sections prepared from the same slabs of bone failed to show much indication of these features. Study of the sections did, however, allow the pattern of cellular activity to be determined in any part of the tissue radiographed. Thus in the area shown in Fig. 1, the radiating structures (which in stereoscopic radiographs are seen to be inter-communicating plates and not spicules) consist of 'woven bone' trabeculæ, the surfaces of which are covered with active osteoblasts. In the less radio-opaque zone deep to this region, extensive osteoclastic activity is apparent, while in the still deeper area of trabecular re-arrangement, lamellar bone-formation is an additional feature.

In Fig. 1, the period of time covered by the development of the periosteal bone was approximately two months. As seven distinct zones can be identified it appears that in this lesion the intermittent process



of periosteal activity is repeated at intervals of about one week.

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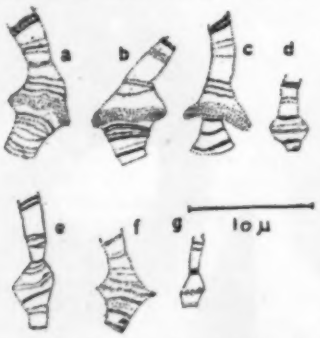
Difference in Structure between the same Giant Chromosomes from the same Larvæ of *Drosophila repleta*

In a previous paper we have shown that the pattern of the same giant chromosomes can differ from cell to cell even in the same tissue of a *Chironomus* larva[1]. Furthermore, the same giant chromosome structure in different tissues shows normally a different pattern[2–6]. The latter differences are clearly greater than those of the chromosome in the same tissue. On the other hand, an examination of giant chromosomes in the mid-gut and in the salivary gland of *Sciara* larva led Berger to the conclusion that the pattern of the mid-gut chromosomes is identical with that of the salivary gland chromosomes[7]. I have investigated both these points with a different object, namely, *Drosophila repleta*. Giant chromosomes closely resembling those of the salivary gland can be found in various somatic tissues in the larvæ of *D. repleta*. They are, however, smaller and have fewer cross-bands than the salivary gland chromosomes. The same phenomenon was observed in *D. virilis* by Makino[8]. Besides the salivary gland chromosomes, those in the cells at the base of the large tracheal tubes give the most satisfactory results.

I have compared the same free end of one of the long chromosomes. The figure shows that the same region does not always have the same number of cross-bands in all nuclei, either of the salivary gland or of the tracheal cell. Only some cross-bands are regularly present in these chromosomes in each tissue; but more can be found if the giant chromosomes are especially enlarged. If we compare the salivary gland chromosomes with those of the tracheal cells, we see that the former usually have more cross-bands than the same chromosomes in the tracheal cells (Figs. *a*—*d* and *e*—*g*). On the other hand, a small salivary gland chromosome often has the same pattern as a large tracheal chromosome (Figs. *c* and *e*).

Hence both salivary gland and tracheal chromosomes develop with the same trend; but the process that leads to the formation of cross-bands goes on more quickly and more strongly in the salivary gland chromosomes than in the others. This causes the different pattern in the same chromosome in the two tissues.



Salivary gland chromosomes (*a*—*d*), and tracheal chromosomes at the base of the large tracheal tubes (*e*—*g*)

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Jan. 8.

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A Labyrinthodont from the Trias of Bear Island, Spitsbergen

During the course of an ecological survey of Bear Island in the summer of 1948, a nearly complete labyrinthodont skeleton was found lying horizontally in the fissile shales of the Mount Misery plateau. These shales are believed to belong to the Upper Triassic, and the overlying "Myophoria Sandstone" has a rich marine fauna of Carnian age[1]. The site was about 430 m. above sea-level on the southern slopes of Urd (the highest point on the island and the most southerly of the three peaks of Mount Misery), about 100 m. below the peak and very close to the edge of the steep slopes falling to the sea on the southern side.

The whole fossil, which appears to be that of a large brachyopid, measured approximately 2·6 m. from the tip of the nose to the last preserved caudal centrum. The skull measured 70 cm. across the horns, and 21 cm. from the tip of the nose to the back of the dorsal surface of the skull in the mid-line. Twenty-four ribs were exposed on the left side and nineteen on the right. A nearly complete rib from the thoracic region measured 30 cm. in length. Some parts of the limb girdles were seen, and the remains of about thirty vertebræ. Approximately 45 cm. of the caudal region was preserved.

All the bones were broken into a large number of fragments, and with the available equipment the fossil could not be collected. In view of the almost certain loss which would result from the oncoming winter weather, sample bones were collected and certain parts were embedded in paraffin-wax (the only available embedding material). Photographs taken by Mr. N. Creasey gave considerable information concerning the proportions of the skull and body.

Consideration of the available data indicates that the animal was a plagiosaur with skull proportions much like those of *Gerrothorax*[2] but of relatively enormous size.

It is hoped that a Norwegian expedition will be able to visit Bear Island in an endeavour to find more and possibly better material.

I wish to thank Mr. F. R. Parrington for his interest in this find, and to acknowledge the help given by Mr. E. Duffey and Mr. D. E. Sergeant in the collection of the material.

J. Lowy

Zoological Laboratory,
Cambridge. Jan. 15.

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TRYPSIN SPLITTING AND DENATURATION OF β-LACTOGLOBULIN

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LINDERSTRØM-LANG, Hotchkiss and Johansen[1] have stressed the importance of denaturation in the proteolytic splitting of globular proteins. They suppose that the process genuine⇌denatured is forced in the direction of the right-hand side of the equation by splitting of the D-form, both forms gradually disappearing; and they suggest that the peptide bonds as such are masked in native globular proteins, and first 'appear' on denaturation.

More recently, Linderstrøm-Lang, Jacobsen *et al.* have found further support for the correctness of this view (unpublished investigations on volume changes in hydrolytic splitting). An interesting paper by Lundgren[2] similarly confirms the theory convincingly. By experiments with ultra-centrifugation and electrophoresis, he showed that active papain catalyses the denaturation of thyroglobulin before its splitting.

While working with other problems, it was observed that β-lactoglobulin is much more easily split by trypsin when a small quantity of urea, which in itself has only a very slight denaturing effect on the protein, is added. It was therefore thought to be of interest to investigate further the way in which trypsin acts in such comparatively weakly denaturing systems.

Experimental. It was found that the processes occurring (denaturation and splitting of peptide bonds) can be followed most easily and instructively by observing the changes in optical rotation. In all the experiments presented here, the concentration of lactoglobulin was 2·17 per cent and the temperature was 30° C. Ammonium–ammonia buffer systems were used, and *p*H was controlled by use of the glass electrode. The trypsin concentrations indicated are based on moist filter-cake. (Percentage trypsin times 0·41 gives actual enzyme concentration in all cases.) By use of three-branched mixing tubes, protein, enzyme and denaturing agent were kept separate until the moment of mixing. The process of denaturation was checked by estimating the proportion of protein insoluble in a salt-buffer mixture of previously given composition[3].

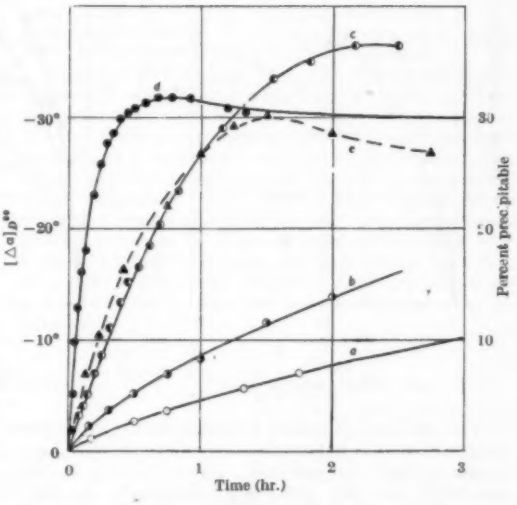

Fig. 2
a, Without trypsin, *p*H 9·25
b, 0·001 per cent trypsin, *p*H 9·25
c, 0·01 „ „ „ „
d, 0·1 „ „ „ „
e, See text

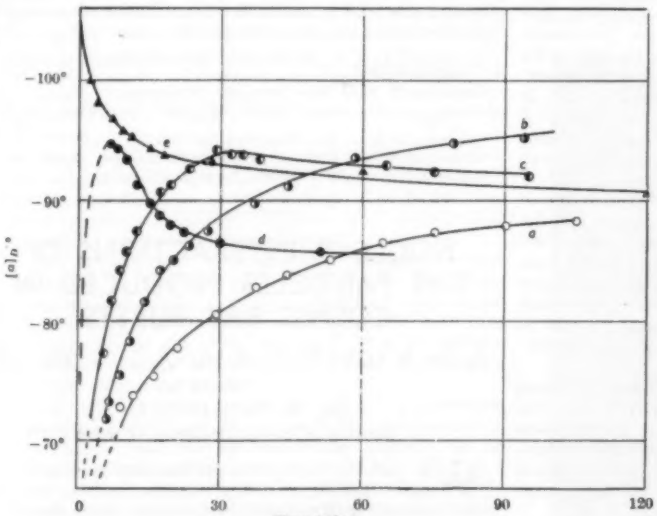

Fig. 1
a, Without trypsin
b, 0·002 per cent „
c, 0·01 „ „
d, 0·1 „ „ „
e, 0·1 „ „ „

Curve *a*, Fig. 1, shows how a progressive denaturation of β-lactoglobulin is accompanied by a considerable increase in lævorotation, while curve *e* shows that the splitting of completely (urea-) denatured β-lactoglobulin is accompanied by a comparatively small decrease in rotation. Curve *a* refers to the following system: 2·17 per cent lactoglobulin, 19 per cent urea, 1/10 *N* ammonium–ammonia, *p*H 8·25. Curves *b*, *c* and *d* show that the addition of trypsin to this system has a pronounced 'catalytic' effect on the rate of denaturation. On addition of 0·1 per cent trypsin, the process is especially illuminating. In this case the denaturation is extremely rapid and almost complete, and is 'followed' by splitting of the denatured protein (compare with curve *e* in Fig. 1).

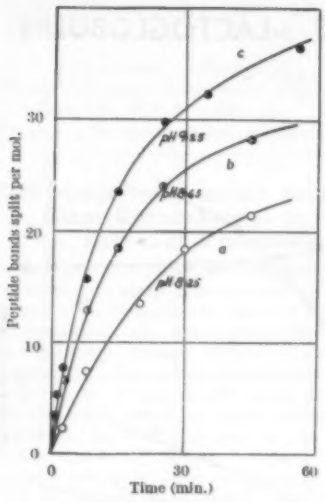

Fig. 3
a, *b* and *c*, with 0·1 per cent trypsin

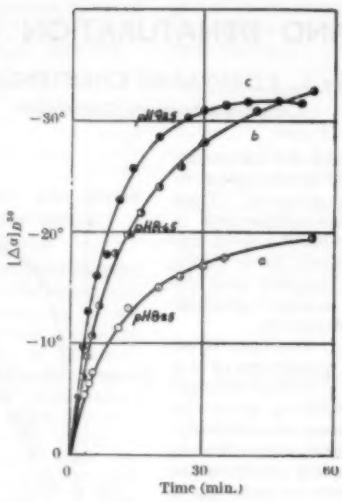

Fig. 4
a, *b* and *c*, with 0·1 per cent trypsin

The splitting of native β-lactoglobulin (in systems without urea) is accompanied by an increase in lævo-rotation (see, for example, curve *a*, Fig. 4), and according to the preceding paragraph it seems natural to regard this as an indication that the process of denaturation (with a large increase in rotation) precedes the splitting of peptide bonds (accompanied by a relatively small decrease in lævo-rotation).

In order to elucidate this further and to see if, under these conditions, there is a similar interaction between the enzyme and the alkaline environment, the stability of β-lactoglobulin in this medium was investigated. At *p*H 8·25 a slow denaturation was found (measured by increasing lævo-rotation and insolubility in the salt-buffer mixture previously described), and the speed was found to increase with rising *p*H. Curve *a*, Fig. 2, shows this alkali denaturation at *p*H 9·25, and by analogy with Fig. 1 it is further shown how increasing concentrations of trypsin catalyse this process. Control experiments with heat-treated trypsin (0·01 per cent) gave a curve exactly like curve *a*.

The dotted curve in Fig. 2 shows the percentage of total protein precipitated at different times by the salt-buffer mixture; the curve has been constructed from analysis of samples removed at intervals from a system corresponding to curve *c* of Fig. 2 (0·01 per cent trypsin). The curve gives the direct proof that, under these conditions, more denatured protein is produced than the enzyme can break down immediately to unprecipitable products.

Fig. 3 shows how the rate of splitting increases with rising *p*H values (three values given). On the basis of the experiments described above, the probable explanation for this is that the pure alkali denaturation, and consequently the alkali denaturation catalysed by trypsin, proceeds more quickly as the medium becomes more alkaline (Fig. 4).

According to Northrop[4], the degree of ionization of the protein substrate determines the optimum *p*H for splitting, an explanation which Bergmann and Fruton[5] from their investigations on the breakdown of synthetic substrates have described as inadequate. In the case of β-lactoglobulin, alkali denaturation will, of course, be of great importance in determining the optimum *p*H (this will naturally also be dependent on the stability of the trypsin, and as additional experiments seem to show, among other things also on the concentration of the trypsin and on the temperature chosen). The observation[6] that the optimum *p*H for the trypsin splitting of denatured protein is lower than for breakdown of the same in the native state may support the view given above. So far it is uncertain if alkali and acid stability have the same importance for the proteolytic hydrolysis of other native proteins.

It was thought to be interesting to compare β-lactoglobulin with trypsin-resistant proteins such as native serum albumin and egg albumin, which are easily split in the denatured state. In contrast to lactoglobulin, these are not easily denatured by alkali (standing several days at *p*H 9). Neither was trypsin found to have any catalytic effect on the denaturation of egg albumin in a weakly denaturing system (25 per cent urea, *p*H 9·25), and both conditions for splitting were thus missing here.

The results given here are in agreement with the hypothesis proposed by Linderstrøm-Lang *et al.* for denaturation as the initial process in trypsin splitting. They demonstrate, however, that the mechanism of this process is more complicated than these investigators had supposed, and a further analysis cannot be given before additional experimental material is available. I am indebted to Prof. Linderstrøm-Lang for valuable criticism.

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NUCLEAR INTERACTIONS OF THE PARTICLES PRODUCED IN COSMIC RAY BURSTS

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WE wish to report the preliminary results of an investigation to study nuclear bursts, carried out recently with a counter-controlled cloud chamber at the Laboratorio della Testa Grigia (3,500 m.).

By a 'nuclear burst' we mean any event in which a particle (ionizing or not), by interacting with a nucleus, gives rise to new ionizing particles, generally different in nature and in energy. In this definition

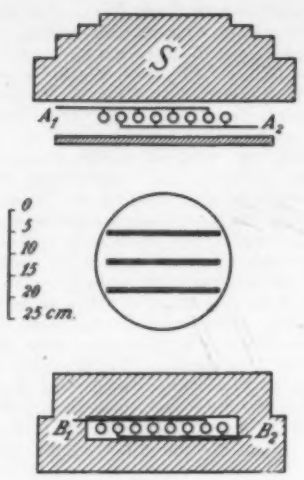

Fig. 1

unlike events such as stars, penetrating showers, mixed showers, etc., are included[1].

The arrangement used is represented in Fig. 1. The cloud chamber expanded whenever one counter at least was discharged in each of the groups A_1, A_2, B_1, B_2, and for this two particles at least were necessary. The cloud chamber was 32 cm. in diameter with an observable depth of 10 cm. It contained three 1·2-cm. lead plates, and was filled with argon at about 85 cm. pressure. Stereoscopic photographs were taken.

In 205 hours, 801 photographs were obtained, from which we selected those which appeared interesting owing to the presence in the cloud chamber of: (*a*) groups of particles originating from a common point in the lead S (Fig. 1), or in the three plates inside the chamber, and in which penetrating particles (recognizable as not generating secondaries in any plate) or heavy ionizing ones were present; (*b*) showers with no evidence of penetrating particles but certainly descending from S, so that the showers appeared under 23 cm. of lead (referred to as electron showers).

Thirty-eight photographs present showers of penetrating particles only (penetrating showers), produced in the lead S; nine mixed showers, that is, showers that certainly contain electrons and penetrating particles; sixteen electron showers.

In twenty-two photographs, nuclear bursts appeared produced in the lead plates of the cloud chamber associated or not (thirteen and nine respectively) with local showers produced in S and recognizable in the cloud chamber. Six of thirteen associated bursts clearly show the penetrating particle producing them.

The study of the properties of the penetrating particles of the bursts in regard to the production of new bursts and to the scattering permits us to formulate some conclusions.

Successive multiple production. Penetrating showers observed by us are nearly all produced in the deeper region of S. Moreover, we observe that the penetrating particles of a penetrating shower seen in the cloud chamber are normally not the same as those which have discharged A_1 and A_2. This is in accord with the hypothesis that the energetic bursts which give rise to the so-called penetrating showers generally contain a large number of short-range particles, too. This agrees with a recent suggestion of Powell and co-workers[2], and confirms that the penetrating showers and the bursts observed in the cloud chamber are of the same fundamental structure, eventually differing only in energy. Moreover, burst production has been found to be the principal interaction of the penetrating particles of the local showers produced in S, with the plates inside the cloud chamber.

Therefore, we are led to a sort of cascade representation[3], in which the most energetic of the particles produced may successively give rise to new bursts. We can make an attempt to clear up this point with the following estimate. In the 46 penetrating and mixed showers observed, we counted 142 particles which traversed the 1·2-cm. lead plates inside the cloud chamber a total of 380 times; six penetrating particles of these showers produced a new burst, with heavy ionizing and penetrating particles, in a plate of the cloud chamber. Hence we obtained a mean cross-section for the production of new bursts:

$$\sigma = \frac{6}{380 \times 1{\cdot}2 \times n} \frac{1}{p} = \frac{4 \times 10^{-25}}{p} \text{ cm.}^2 \text{ per nucleus,}$$

where n is the number of lead nuclei per cm.^{3}, and p is a mean probability that a burst produced within the plate may still appear in the cloud chamber with heavily ionizing particles.

The value of p is uncertain, for its average value has certainly to be extended to very wide energy limits. On the basis of recent work[4,5], we have estimated that a value $p \sim 0{\cdot}4$ may be suitable. With this value we obtain $\sigma \sim 10^{-24}$ cm.^{2}. It is interesting to note that this value of σ is not far from the geometrical cross-section of the lead nucleus ($2{\cdot}5 \times 10^{-24}$ cm.^{2}), and is near the value already established for the particles of the total component which produces bursts[6].

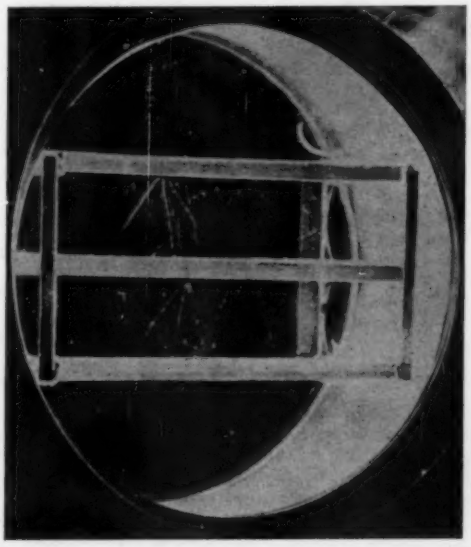

Fig. 2

Our results show, moreover, that, in the absorption measurements of the radiation which produces nuclear disintegrations[4], a large percentage of the events observed (stars and bursts) are produced by the secondaries generated in the absorbers.

Scattering. We have searched for other interactions of the penetrating particles of the bursts, that is, for scattering. To do this we measured the scattering angles through the three plates of thirty-eight penetrating particles associated in bursts produced in S in a manner similar to that used by Rochester[7]. These tracks were found on photographs selected *a priori* owing to their being technically good. The scattering observed is never larger than 6°, except in one case (9°), and can be explained by Coulomb scattering alone, without any anomalous scattering. One scattering of $\sim 25°$, on the contrary, has been observed in penetrating particles not clearly associated in bursts.

A typical photograph of a burst produced by a penetrating particle of a penetrating shower is reproduced in Fig. 2.

A detailed report will be submitted to the *Nuovo Cimento*. We are greatly indebted to Prof. B. Ferretti for helpful discussions.

[1] Rossi, B., *Rev. Mod. Phys.*, **20**, 537 (1948), and references cited there.
[2] Brown, R., Camerini, U., Fowler, P. H., Muirhead, H., Powell, C. F., and Ritson, D. M., *Nature*, **163**, 47 (1949).
[3] The existence of successive multiple nuclear disintegrations is referred to in Bridge, H. S., Hazen, W. E., Rossi, B., and Williams, R. W., *Phys. Rev.*, **74**, 1083 (1948).
[4] Bridge, H. S., and co-workers, *loc. cit.*
[5] Rochester, G. D., and Butler, C. C., *Proc. Phys. Soc.*, **61**, 307 (1948).
[6] Jánossy, L., and Rochester, G. D., *Proc. Roy. Soc.*, A, **183**, 181 (1945). Meyer, H. A., Schwachheim, G., and Wataghin, A., *Phys. Rev.*, **74**, 846 (1948). Cocconi, G. (unpublished).
[7] Rochester, G. D., *Proc. Roy. Soc.*, A, **187**, 464 (1946).

DISSOCIATION EXTRACTION

By Dr. G. H. TWIGG

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A PROCESS has been devised for separating organic acids (or bases) of different dissociation constants from each other by solvent extraction. It is based on the fact that, in general, weak organic acids are soluble in solvents only in the form of the free undissociated acid. In essence, the system consists of a continuous counter-current extraction column in which the solvent flows in one direction (say, upwards) and water flows in the opposite direction. The mixture to be separated is fed to an intermediate point in the column. The acids partition themselves between the solvent and the water in a way which depends on the dissociation constant (K) and the pH. The concentration of an acid in the solvent (C_S) is related to its concentration in the water (C_W) by the equation

$$\frac{C_S}{C_W} = S.\frac{[H^+]}{[H^+] + K}, \quad . \quad . \quad (1)$$

where S is the partition ratio of the acid between the solvent and water. The weaker acid is preferentially present in the solvent; for example, if the partition ratios of the two acids are equal and the first acid is taken as the weaker ($K_1 < K_2$), then

$$\frac{C_{1S}}{C_{2S}}\Big/\frac{C_{1W}}{C_{2W}} = \frac{[H^+] + K_2}{[H^+] + K_1}. \quad . \quad . \quad (2)$$

This equation shows that the degree of separation can be improved up to a maximum value by reducing the hydrogen ion concentration. There is, however, a practical limit to this as it entails a reduction in the concentration of acid in the solvent (equation 1); on the other hand, this can be to some extent counterbalanced by suitable choice of solvent, which should be one with a high partition ratio.

The solvent as it flows upwards is, by repeated contact, progressively enriched in the weaker acid, and correspondingly the aqueous phase is enriched in the stronger acid. The column is operated so that the total concentration of acid in each phase is constant; thus, since the weaker acid is segregated to the top of the column, the pH is higher there than at the bottom, and there is a continuous pH gradient along the length of the column. Reflux is applied at both ends of the column. At the top it is effected by adding alkali with the ingoing water, thus removing part of the acid from the outgoing solvent. At the bottom, strong mineral acid is added with the solvent in order to displace some of the organic acid from the aqueous phase to the solvent. The condition of constancy of total acid concentration in each phase follows from the fact that additions of mineral acid and alkali are made only at the ends of the column and with the feed.

This column bears a considerable resemblance to a continuous distillation column. The material to be separated consists here of two acids of different dissociation constants, in place of two liquids of different volatilities. Both processes involve the transfer of material from one phase to another, followed by separation of the phases. The solvent phase is analogous to the vapour phase in distillation, and the weaker acid which is preferentially present in the solvent phase is analogous to the more volatile component. The process of transfer from aqueous phase to solvent is paralleled by the process of vaporization. The greater the hydrogen ion concentration in the aqueous phase, the greater will become the acid concentration in the solvent phase; in the case of distillation, the higher the temperature of the liquid, the greater becomes the pressure (or concentration) in the vapour phase. Thus temperature in distillation corresponds to hydrogen ion concentration. The weaker the acid, the more readily it passes into the solvent phase at a given pH; that is, a weak acid with a low value for the dissociation constant corresponds to a volatile liquid of low boiling point. The ratio on the left-hand side of equation 2 has the same meaning as the volatility ratio. Application of reflux by addition of alkali with the water corresponds to the use of a dephlegmator in distillation.

The operating conditions of the column can be calculated along similar lines to those used in distillation. An equilibrium curve can be calculated, knowing the partition ratios and the dissociation constants, or it may be determined experimentally. From this, by using a construction similar to the McCabe–Thiele construction for distillation, the number of theoretical stages required to effect a given separation can be deduced.

The analogy developed here is with continuous distillation; a similar analogy may be drawn between batch distillation and the Craig[1] separation process, which likewise depends on differences in partition ratio and dissociation constant.

It is proposed to term this process 'dissociation extraction'. A full account of this work will be

published elsewhere. The process appears in essence to have been independently discovered elsewhere; it is described in a patent[2] which does not deal with the theory of the process, and which was published after the present work had been completed.

I am indebted to the directors of the Distillers Company, Ltd., for permission to publish this work.

Craig, L. C., *J. Biol. Chem.*, **155**, 519 (1944).
[2] B.P. 599,854.

RESEARCH GRANTS IN BELGIUM

THE annual report of the Institute for the Encouragement of Scientific Research in Industry and Agriculture, Belgium, for 1948, in addition to the budget and accounts and a list of members of Council, details the grants made during the year, with some notes on the researches in progress. Of thirty-two new grants, only one was to an individual investigator. The National Centre for Metallurgical Research at Hainault received 9,466,000 francs for metallurgical research, which during the last two years has been concerned with the determination of gases in metals, the spectrographical determination of carbon and phosphorus in steel, the study of the influence of the grain of steel on its weldability, the heterogeneity of ingots, the conditioning and control of the charge in blast furnaces, the influence of the chemical composition of the melt on the behaviour of the ingots, the micro-analysis of steels and X-ray diffraction studies. The Committee for the Establishment of a Pedological Map of Belgium received 8,085,000 francs for the systematic investigation of the pedological characteristics of Belgian soils; and the Electrical Construction Works of Charleroi received 6,550,000 francs for investigations on applications of ultra-short waves, ultrasonics, the recording of over-voltage, Geiger–Müller tubes and counters, electronic medical appliances and sources of ions, all directed to the development in Belgium of a new branch of the electrical industry.

The National Centre for Research on Pastures and Fodder was granted 3,561,000 francs for its work on the phytosociological analysis of pastures in different agricultural regions, the regeneration of pasture by deep aeration following partial seeding, the control or elimination of weeds, including the use of calcium cyanamide, and on the rational and intensive exploitation of pastures and fodder at the four experimental stations of Berlaer, Herve, Leval and Eccloo. The Centre for the Study of High Polymers received 2,892,800 francs for research on the kinetics of polymerization and polycondensation, the characterization of different fractions of a high polymer, on the statistical form of the molecules, the behaviour of the molecule in solution, and cellulose ethers and esters. The Foundation for Potato Research received 2,545,000 francs for work on genetical selection and the breeding of selected varieties. To the Committee for the Scientific and Technical Study of Milk and its Products 2,490,000 francs was given for work on the improvement of the quality of milk and butter, and to the Belgian Institute for the Improvement of the Beetroot 2,352,500 francs for agronomical, chemical and biological research on the cultivation of the sugar-beet and for the study of its mechanization. The National Institute for the Improvement of the Conservation of Vegetables received 1,615,000 francs for research on the cultivation of such crops, the selection of varieties, and the biological, chemical and physical conditions of preservation.

The Belgian Institute for High Pressures received 1,346,000 francs for both static and dynamic work at high pressures, work on explosives, spectroscopical work, and biological work at high pressures. The Belgian Committee for Electrochemistry and Thermoelectricity received 1,121,800 francs for research on induction furnaces and their use in metallurgy; and to the Committee for the Study of Creep of Metals at Ordinary Temperatures 1,037,000 francs was granted for research on the creep of high-resistance steels. Smaller grants were made to the National Centre for Animal Research, the Photo-Products Gevaert (for the study of emulsions), the Centre for Child Care and Pediatry, the Union for Electrical Development in Belgium (for research on high-pressure steam pipes), the Belgian Association for the Study, Testing and Use of Materials for investigations on the protection of steel by paint, the Bureau of Physico-Chemical Standards for metrological research on standards, the Belgian Institute of Welding, the National Committee for the Investigation of Penicillin and the Committee for Physico-chemical Research at Low Temperatures.

AMERICAN PHYSICAL SOCIETY

ANNUAL MEETING

THE 1948 annual meeting of the American Physical Society, the largest meeting in its history, was held at Columbia University, New York City, during January 26–29. The retiring presidential address was delivered by Prof. J. R. Oppenheimer, who took as his subject "Fields and Quanta"; and the after-dinner speeches, "Sentimental Democracy and the Forgotten Physicist" and "Freedom versus Security in the Modern World", were made by Profs. P. W. Bridgman and R. E. Cushman, respectively. The text of Prof. Cushman's speech is reprinted in the March issue of *Physics Today*.

At the business meeting on January 28 the following officers were elected: *President*, F. W. Loomis; *Vice-President*, I. I. Rabi; *Secretary*, K. K. Darrow; *Treasurer*, G. B. Pegram; *Members of Council* (four-year terms), L. W. Alvarez and V. F. Weiskopf; *Members of the Board of Editors* (three-year terms), J. Bardeen, R. G. Herb and W. E. Lamb, jun.

The total number of papers contributed to the meeting was three hundred and eight. Most of these were ten-minute papers; but there were a few invited papers. These included: in the general programme, papers by L. Brillouin on the interaction between waves and electrons travelling together, and by H. Yukawa on models and methods in meson theory; in the division of electron physics, W. Shockley on the electronic theory of the transistor; and in the division of fluid dynamics, T. von Kármán and C. C. Lin on the statistical theory of isotropic turbulence, and M. Schwarzschild on turbulence in the atmosphere of stars. A complete list of the papers, together with abstracts, is given in the minutes of the meeting, which are printed in the April 15 issue of the *Physical Review* (**75**, 1279–1338; 1949).

At the Council meeting it was reported that the financial position of the Society was unsatisfactory. It was certain that nearly half the accumulated

surplus of the Society would disappear during 1949. This was due to the very large size of the *Physical Review*, which, although the number of pages per member was still less than before the War, would incur a large deficit because of rising printing costs. The Council recommended an immediate increase in member subscriptions with a possible further increase later, and decided also to discontinue the publication in the *Physical Review* of abstracts of contributed papers presented at meetings.

The eighteenth annual meeting of the American Association of Physics Teachers was held together with the American Physical Society meeting. Prof. H. K. Schilling acted as chairman of the programme committee. The Oersted Medal, which was awarded to Prof. Arnold Sommerfeld, was accepted on his behalf by Dr. E. U. Condon. The seventh Richtmyer Memorial Lecture was delivered by Dr. L. A. DuBridge, who spoke on "How World War II has affected the Science of Physics". Two discussions, on physics and general education, and on the training of college teachers of physics, and a symposium on testing in physics teaching formed the main programme of the meeting.

FORTHCOMING EVENTS

*(Meeting marked with an asterisk * is open to the public)*

Tuesday, June 28

ROYAL ANTHROPOLOGICAL INSTITUTE (at 21 Bedford Square, London, W.C.1), at 5 p.m.—Annual General Meeting.

INSTITUTE OF SOCIOLOGY (at the Royal Institute of British Architects, Portland Place, London, W.1), at 5.30 p.m.—Mr. Roger Wilson: "Aims and Methods in a Department of Social Studies".

Tuesday, June 28—Thursday, June 30

INSTITUTION OF NAVAL ARCHITECTS (at the Royal Society of Edinburgh, 22 George Street, Edinburgh).—Summer Meeting.

Wednesday, June 29

GEOLOGICAL SOCIETY OF LONDON (at Burlington House, Piccadilly, London, W.1), at 5 p.m.—Dr. E. R. Gee: "Problem of the Saline Series, Salt Range, Punjab".

Thursday, June 30

ROYAL SOCIETY (at Burlington House, Piccadilly, London, W.1), at 4.30 p.m.—Dr. D. W. Bronk, For.Mem.R.S.: "The Rhythmic Action and Respiration of Nerve Cells" (Croonian Lecture).

Thursday, June 30—Saturday, July 2

ROYAL ASTRONOMICAL SOCIETY (joint meeting with the PHYSICS DEPARTMENT of the UNIVERSITY OF MANCHESTER and the MANCHESTER ASTRONOMICAL SOCIETY, at the University, Oxford Road, Manchester).

Thursday, June 30

At 8 p.m.—Dr. M. A. Ellison: "Sun, Radio and the Stars".*

Friday, July 1

At 4.30 p.m.—Scientific Papers.

Saturday, July 2

At 10 a.m.—Geophysical Discussion.

Friday, July 1

GEOLOGISTS' ASSOCIATION (at the Geological Society, Burlington House, Piccadilly, London, W.1), at 6 p.m.—Mr. B. W. Sparks: "The Denudation Chronology of the Dip-Slope of the South Downs".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

SENIOR SCIENTIFIC OFFICER in the Agricultural Entomology Research Division of the Ministry of Agriculture for Northern Ireland—The Secretary, Civil Service Commission, Stormont, Belfast (June 30).

ASSISTANTS (2) IN MATHEMATICS—The Secretary, The University, Aberdeen (June 30).

ASSISTANT LECTURER IN MATHEMATICS (Applied Mathematics)—The Principal, Royal Holloway College, Englefield Green, Surrey (July 1).

RESEARCH SCHOLARSHIP IN VERTEBRATE ZOOLOGY—The Registrar, The University, Bristol 8 (July 2).

LECTURER IN PHYSICS up to B.Sc. standard—The Clerk to the Governors, Chelsea Polytechnic, Manresa Road, London, S.W.3 (July 2).

LECTURER IN GEOPHYSICS in the Department of Geology—The Secretary, The University, Birmingham 3 (July 2).

ASSISTANT LECTURER IN PHYSICS in the Bradford Technical College—The Director of Education, Town Hall, Bradford (July 2).

LECTURER IN GEOLOGY—The Secretary, The Durham Colleges, 38 North Bailey, Durham (July 4).

PROFESSOR OF PHYSICS, a PROFESSOR OF MATHEMATICS, and a READER IN GEOLOGY, at the University of Dacca, East Bengal, Pakistan—The High Commissioner for Pakistan, 14 Fitzhardinge Street, London, W.1 (July 7).

HISTOLOGIST to collaborate in researches on endocrinology of lactation and reproduction and other research projects involving histological studies—The Secretary, National Institute for Research in Dairying, Shinfield, Reading, Berks (July 9).

LECTURER IN BIOLOGY, to teach to Degree standard—The Registrar, Nottingham and District Technical College, Nottingham (July 11).

MUSEUM ASSISTANTS—The Museum Superintendent, Science Museum, South Kensington, London, S.W.7 (July 15).

ASSISTANT LECTURER, and a DEMONSTRATOR, IN ZOOLOGY—The Registrar, The University, Leeds 2 (July 18).

LIBRARIAN to the Ministry of Food—The Secretary, Civil Service Commission, 6 Burlington Gardens, London, W.1, quoting No. 2014 (July 30).

ELECTRICAL, ELECTRONIC ENGINEERS (Ref. D6/49A), and MECHANICAL AND AERONAUTICAL ENGINEERS (Ref. C12/49A), in Senior Experimental Officer, Experimental Officer and Assistant Experimental Officer grades at the Royal Aircraft Establishment, Farnborough, and other Ministry of Supply Research and Development Establishments in S. England and the Midlands—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting the appropriate Ref. No. (July 30).

ASSISTANT LECTURER AND DEMONSTRATOR IN THE DEPARTMENT OF PHYSICS—The Secretary, King's College of Household and Social Science, Campden Hill Road, London, W.8.

TECHNICAL OFFICER to work under the Seed Production Committee on root and vegetable seed crops—The Secretary, National Institute of Agricultural Botany, Huntingdon Road, Cambridge.

SENIOR LECTURER IN PHYSICS at the Karachi Women's College—The Educational Attaché, High Commissioner for Pakistan, 14 Fitzhardinge Street, London, W.1.

LECTURER IN METALLURGY—The Principal, Borough Polytechnic, Borough Road, London, S.E.1.

ASSISTANT LECTURER IN PHARMACEUTICAL CHEMISTRY—The Registrar, The University, Nottingham.

DIRECTOR OF THE WEST AFRICAN FISHERIES RESEARCH INSTITUTE, Sierra Leone—The Under-Secretary of State, Colonial Office (Research Department), Sanctuary Buildings, Great Smith Street, London, S.W.1.

LECTURER IN AGRICULTURE (Crop Husbandry and Farm Management), a LECTURER IN HORTICULTURE (Pomology and Orchard Management), a LECTURER IN AGRICULTURAL CHEMISTRY, an ASSISTANT LECTURER IN STATISTICAL THEORY AND EXPERIMENTAL METHOD, an ASSISTANT RESEARCH CHEMIST (analytical), and an ASSISTANT PLANT BREEDER in the Hop Research Department—The Registrar, Wye College, Wye, Ashford, Kent.

RESEARCH PATHOLOGIST to the Ophthalmological Research Unit—The Secretary, Royal College of Surgeons, Lincoln's Inn Fields, London, W.C.2.

LECTURER IN MATHEMATICS, to teach Intermediate and Final Degree students—The Clerk to the Governing Body, Battersea Polytechnic, Battersea, London, S.W.11.

SENIOR TECHNICIAN, and TECHNICIANS (3)—The Regional Transfusion Officer, Regional Blood Transfusion Service Laboratories, Northfield Road, Sheffield 10.

GLASS-BLOWER, with special knowledge of vacuum technique, for research in Physics—The Secretary, Queen's University, Belfast.

ASSISTANT for scientific abstracting and information service—The Director, Commonwealth Bureau of Animal Breeding and Genetics, King's Buildings, West Mains Road, Edinburgh 9.

ASSISTANT OR ASSOCIATE PROFESSORSHIPS OF BIOCHEMISTRY and of GENERAL AND ANALYTICAL CHEMISTRY—The Secretary, Department of Chemistry, University of British Columbia, Vancouver, B.C., Canada.

ASSISTANT MANAGER (Production), to be responsible to the Production Manager for the operation of a large chemical plant—The Staff Section, Ministry of Supply Division of Atomic Energy (Production), Risley, Warrington.

METALLURGISTS (2, Scientific Officer grade, one specialist in light alloys and one in ferrous metals), ENGINEERS (2, one Senior Experimental Officer grade and one Experimental Officer grade), and a PHYSICIST (Senior Scientific Officer grade)—The Secretary, British Welding Research Association, 29 Park Crescent, London, W.1.

SCIENTIFIC ASSISTANT IN THE INFORMATION DEPARTMENT—The Information Officer, British Scientific Instrument Research Association, 17 Princes Gate, London, S.W.7.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Visual Education Film Strips. Autumn Fruits (Poisonous). By Rodney F. Cosser. Pp. 12+film strip. 21*s*. Autumn Fruits (Non-Poisonous). By Rodney F. Cosser. Pp. 12+film strip. 21*s*. An Introduction to Fungi, Part 1. By Rodney F. Cosser. Pp. 12+film strip. 21*s*. An Introduction to Fungi, Part 2. By Rodney F. Cosser. Pp. 10+film strip. 21*s*. (London: *Daily Mail* School-Aid Department, 1949.)

Geological Survey of Great Britain: Scotland. Wartime Pamphlet No. 9: Talc and other Magnesium Minerals and Chromite associated with British Serpentines, Supplement No. 1: The Corrycharmaig Serpentine Intrusion, Glen Lochay, Perthshire. By Dr. J. G. C. Anderson, Dr. K. C. Dunham and C. O. Harvey. Pp. 14. (London: Geological Survey and Museum, 1949.) 9*d*. [105